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May, 1913

THE  
**Psychological Review**

EDITED BY

JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY  
HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)  
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*) AND  
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# THE PSYCHOLOGICAL REVIEW

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## THE RELATION OF PSYCHOLOGY TO PHILOSOPHY AND EDUCATION<sup>1</sup>

BY ROBERT MORRIS OGDEN

*University of Tennessee*

The theme of my address I have found ready-made in the aim and purpose of our Society. Yet I have considered it expedient to treat the matter of these relations from the standpoint of psychology. In doing this, it is, I hope, needless for me to say that I have had no thought of depreciating the high position occupied by philosophy, our mother of sciences, nor of robbing the field of education of its well-earned claim to independence. I have simply been actuated by a knowledge of my personal limitations in attempting to deal with these disciplines from any other basis than that of pure psychology.

The relations existing in American universities between philosophy and psychology present a marked contrast with those which obtain in Germany, where psychology had its birth as an independent science. Almost from the beginning separate chairs and departmental independence have been the rule with us, whereas in Germany it is only within the last few years, and notably through the utterances and publications of Külpe<sup>2</sup> and Marbe,<sup>3</sup> that a movement has been set on foot which aims to secure autonomy for psychology.

Judging by the steady growth of psychological literature, in all its phases, which Germany has contributed since the

<sup>1</sup> Presidential address, delivered before the Southern Society for Philosophy and Psychology at the Johns Hopkins University, April 8, 1913.

<sup>2</sup> 'Psychologie und Medizin,' Leipzig, Engelmann, 1912.

<sup>3</sup> 'Die Bedeutung der Psychologie für die übrigen Wissenschaften und die Praxis.' *Fortschritte der Psychologie und ihrer Anwendungen*, 1912, 1.

founding of Wundt's *Philosophische Studien*, as well as by the number of its eminent psychologists holding, nominally at least, chairs of philosophy, one can hardly conclude that the development of the new science has been seriously hindered in the Fatherland. In America, on the contrary, where complete independence has been vigorously sought, and maintained, there has arisen a condition of affairs which, I believe, is not altogether advantageous to psychology. In his recent article on 'The History and Status of Psychology in America,' Ruckmich finds, "that psychology, after over twenty-five years of growth, does not stand very high on the honor roll among other academic subjects."<sup>1</sup> One plea, therefore, which I wish to make on this occasion, and which I hope to justify in some measure, is for a closer affiliation between these two branches of academic activity, not only as regards the articulation of courses in our academic curricula, but also with reference to the best and most profitable procedures of investigation in both fields.

Such a plea has, I believe, an especial cogency at the present time in the south, because of the fact that, as a whole, our institutions are less developed along this line than is the case in other sections of this country, there being, so far as I know, no special chair exclusively for psychology established in any southern university, barring the Hopkins. Not, I would hasten to add, that I regard with disfavor the establishment of such independent positions; quite the contrary, for I believe this step to be one of the most important which remains to be taken in our representative southern institutions. But I hope that this evolution may be in the highest degree profitable, not only to psychology, but also to philosophy and education, with which disciplines the work in psychology is now most intimately associated. In order that this may be the case, a broad sympathy must be engendered in all three fields of endeavor.

The conditions as they exist peculiarly in the south, but also to a considerable extent the country over, are in many respects a menace, not only to the free development of psycho-

<sup>1</sup> *American Journal of Psychology*, 1912, 23, p. 530.

logy as an independent science, but also to the usefulness of psychology in its many applications to philosophy and education. As one of our eminent psychologists has expressed it, psychology is between the upper and the nether millstones of education and philosophy. I cannot but believe that this pressure, exerted on the one hand by philosophy, with its conservative logical methodology; and, on the other hand, by education, with its practical and highly technical problems; is not only a baneful influence in checking the growth of an independent science, but is also proving a disservice to the ultimate achievements of philosophy and education.

The pressure which is exerted by education to the disadvantage of psychology is due, I think, to the demand which educational theorists make for a schematic setting for their theories. Dealing as they do with adjustments of various sorts; emphasizing as they must the learning-process, and individual traits and tendencies; they are prone to adopt a psychological system which is usually biological in its general outlines. A science of behavior is what they need, but a theory which is largely schematic and undemonstrable is all that they have at hand. They do both themselves and psychology an injustice in accepting the theory with as little critical insight as is often manifest. Thus they rest the weight of their practice upon a theoretical substructure which is often flimsy, and in certain particulars may be highly suspect. The persistence of the Herbartian doctrine of *apperception* in current educational theory is an illustration of this incautious emphasis upon a theory which, while it expresses a general fact, as do all theories, is nevertheless both inadequate and false in the explanation which it gives to the fact.

On the whole, however, I do not feel that any great harm is being done by such instances as these to a satisfactory development of either psychology or education. At worst, they are but evidences of a misguided zeal. The advancement of pedagogy, which in the past few years has been so enormous in the establishment of all manner of special professional courses for the training of teachers, has found the demand outstripping the supply. As a result, much of educational

science has been manufactured on the spot out of material which, while not of the first order in proven worth, was nevertheless the only material to be had. Considering by and large the results thus far achieved, one can but marvel at the rapid strides which have been made, and the healthy growth which has been fostered. Yet I suppose no one would care to maintain that a science of Education has thus far been achieved. It is only in the course of becoming, and its ambitions have frequently gone beyond its resources. Psychology is lending many of its best men to the task of creating such a science, and in the hands of these leaders there need be no fears for the ultimate outcome. We need only to guard against a premature loosening of the bonds which now, and for some time to come, must continue to hold educational theory and practice to sound psychological principles. The peculiar conditions which make the maintenance of this connection so important are, first, that psychology itself is still in the formative stage, many of its principles being yet incomplete and insecure; and, second, that only those who are caring for psychology as a pure science can be entrusted to work out principles which will be truly basic for the applied sciences. The danger lies in that impatience among educational theorists and practitioners which leads them to over-emphasize the symptomatic, and to underestimate the real and crying need for fundamentals.

In the case of philosophy the problem is much more difficult. The new pedagogy has not yet found occasion to forswear its allegiance to psychology. Philosophy, on the contrary, with its manifold problems all historically antedating any attempt at an exact science of mind, has tended to hold itself aloof from the psychologist's investigations, treating them with indifference, indulgence or contempt, as the case might seem to warrant.

The reason for this is fairly obvious, when we consider the roots and the results of psychological achievement to date. Emerging, as they have, largely from the physiological and physical laboratories, the problems with which psychology has thus far been principally dealing are problems which have often but a remote bearing upon the metaphysical theories in which philosophy finds a logical culmination.

It was but natural that psychology should make its start as a science with those phenomena which were most directly observable, and most readily correlated with the data of physics and physiology—namely, the sensations. Nor can any one doubt that the most striking achievements of the science have thus far rested in this field. Yet we can also understand why the philosopher must regard a course in psychology which devotes two thirds of its time to a detailed study of the nervous system and the data of sensation, as a questionable introduction to the problems of logic, ethics, æsthetics and metaphysics. Thus there has been dissatisfaction, and a tendency to divorce psychology altogether from the courses in philosophy with which it was formerly so narrowly allied.

If I may presume to judge from a not too comprehensive knowledge of the facts, the following conditions are rather widely prevalent. It will be found, I think, in a considerable number of our universities where the courses referred to are elective, that the introductory psychology which runs for a term or a year, has often a very large membership, this being partly due to its intrinsic interest, and partly due to the requirements made upon students who are preparing themselves to be teachers. In comparison, the advanced courses in psychology have scarcely any students. It would seem from this that the appeal made by psychology is a very special one—not that of a subject which offers any considerable opportunities for general culture. Philosophy, in its turn, has doubtless suffered in the proportionate number of its students by the separation of psychology from its departmental régime. There are ordinarily no such large numbers taking logic, the usual introductory course, as take psychology. On the other hand, the general distribution of students through the various philosophical courses is likely to be much better than in the advanced courses in psychology, thus indicating that philosophy has a real hold upon the students of general culture such as psychology does not at present seem to possess.

It occurs to me that could the introductory course in psychology be related more fittingly to the problems with

which philosophy has to deal, by emphasizing more the psychology of character and reasoning, and less the psychology of sensation and perception, it would serve much better both its own ends and those of the various disciplines allied to it. Introduction would thus be made to the problems of human intelligence which have the greatest intrinsic interest, and the psychological basis might be more effectively laid for the special problems, not only of philosophy, but also of education. Among the large numbers who at present enroll for one course in psychology, never to proceed further, either in this field or in the domain of philosophy, more might thus be interested to pursue their work along both these lines, thus increasing both the numbers and the cultural interests in all these disciplines.

And the time is ripe, I believe, for such a *rapprochement* and readjustment between philosophy and psychology. It will not be necessary to elaborate the bearings of psychology upon philosophical theory in general, for since the writings of Locke, the dominant note of philosophy has undoubtedly been idealistic, with a strong psychological tinge. One cannot read the newer phases of pragmatism, for instance, as presented by a James, a Dewey or a Bergson, without recognizing at once how implicitly psychological they all are. Yet, granted this, what can psychology do, I may be asked, to advance the interests of the modern philosophical point of view?

Is not the fact that philosophical theory is being advanced and maintained by men of little or no experience in the psychological laboratory, ample evidence that philosophers have no great needs which the specially trained psychologist is likely to be able to supply? And if these theories do rest in part upon psychology, do they not rest equally upon biology and physics?

This is indeed the point at which the psychologist must stand and deliver if he would have his discipline accepted as a peculiarly fitting propædeutic to the problems which modern philosophy has before it. I, for one, believe that he is fast approaching the time when he can do this, and revive for his science something of that prestige which was accorded it in the preëxperimental days of the British empiricists.

The new psychology of thought is rich with promise along this line. Fitting achievements but await the daring investigators who, with an eye to the philosophical problems of existence, will direct their laboratory problems into this field. Beginnings have already been made which date from 1900, and the appearance of Marbe's work on the psychology of judgment. To be sure, Marbe's results were chiefly negative, but he builded better than he knew, as one may realize from the long series of subsequent investigations which have applied his method to the study of thought and meaning, will and conduct.

In his admirable summary dealing with the significance of psychology for science and practice in general,<sup>1</sup> Marbe has recently pointed out many specific instances in which philosophers, such as Sigwart, Windelband and Rickert, have enunciated principles concerning judgment, association and the concept of truth which the new psychology of thought has shown to be quite untenable. The time will come, he avers, when the study of psychology will be held to be as necessary for the philosopher as mathematics for the physicist, physics for the chemist, or classical philology for the historian.<sup>2</sup>

In the remainder of my address I wish to consider more closely the bearings which the recent psychology of the thought-processes seems to have upon two rather important problems, one philosophical and one educational.

For the first instance, allow me to refer to its bearing upon that movement which appears at present to occupy, in this country at least, the center of the philosophical stage. I refer, of course, to the new realism. I shall make no attempt to penetrate the intricacies of this movement, for I am too keenly aware of my own limitations in attempting to deal adequately with any metaphysical problem. But the issue provokes a discussion which, in upholding my thesis of the importance of psychology for philosophy, I cannot afford to neglect, inasmuch as the new realists, if I read them aright, are inclined to identify the psychological with the episte-

<sup>1</sup> *L. c.*, p. 5 ff.

<sup>2</sup> *L. c.*, p. 69.

mological point of view, and to deny the priority of both in setting the ultimate problems of philosophy.

Fortunately, I am able to rest my case here largely in the hands of one who is better qualified to speak with authority. It is a fact of no small significance that just as our American realists are presenting conclusions based upon the rather drastic procedure involved in a denial of the claims of epistemology, there should appear the first of a series of volumes entitled 'Die Realisierung,'<sup>1</sup> by Professor Oswald Külpe, which aim to achieve the same result on the authority of an epistemological inquiry. One cannot read the volume called 'The New Realism,' together with Professor Perry's 'Present Philosophical Tendencies,' and Professor Marvin's 'First Book in Metaphysics,' without a keen appreciation of the ingenuity with which these various authors proceed to substantiate their contention that logic is the sole arbiter in the construction of a valid *Weltanschauung*. But were they not so thoroughly convinced that psychology can offer in a strict sense only sensational contents in an associative setting, I fancy they would not be so radical in their polemic against the epistemological point of view. With such a psychology, indeed, one can make but little progress towards a realistic world-view. But it is precisely this conception of mind which the psychological investigation of the thought-processes is proving to be false. It is impossible to analyze a thought-process into sensations, images and their associative connections. The mechanics of such operations as these are far too crude to give adequate account of the process by which meaning is obtained and judgments formed. At least two additional categories of the imageless order are necessary to describe the process completely. In the first place, one finds thought-contents of things known or referred to, which exist in the conscious complex as definitely and concretely as do the images—yet they are not identifiable with imaginal or sensational experiences, as things seen or heard, neither are they word-symbols nor kinesthetic products. Secondly, one discovers *retrospectively* the existence of acts and tendencies which were not

<sup>1</sup> Leipzig, S. Hirzel, 1912.

themselves conscious, but which have nevertheless been clearly operative in determining the course of thought—the selection of this and the rejection of that; in short, the control which a coherent thought-process manifests in its very existence. It is these operations of thinking, these determining acts of the thought-process, which require, in Külpe's opinion, for their full explanation, not merely the direction upon thoughts, images and sensations, but also the assumption of *real* objects, in order that their logical existence may be justified. Psychology, then, contributes as well as does physics or biology to the assumption of real entities, since mental operations are not satisfactorily explained in terms only of conscious contents in associative relationship.

The previous failure of epistemology to record this fact has, according to Külpe,<sup>1</sup> rested upon its neglect of the problem of *realization*. It has passed over this problem and proceeded at once to the problem of the outer-world, which has usually been solved in idealistic terms, because only conscious contents were taken into consideration. As long as it seemed possible to explain these satisfactorily as emanating from sensation, and grouping themselves according to the laws of contiguous experience and similarity, it was, of course, impossible to derive from psychology any adequate basis for a transcendental realism. But as soon as a narrower examination of the thought-processes had shown that the laws of association were an insufficient explanation for conscious collocations, that *thinking* means the monarchical supervision and control of *tasks* and *determining tendencies* which are directed not only upon images and sensations, but also upon thought-contents of an imageless order, and even external objects which have no adequate representatives in consciousness, then it became necessary to revise the psychology of Berkeley and Hume, and to question the old dogma: *Nihil est in intellectu, quod non prius fuerit in sensu*.

Psychology may not give us reality out of hand, but it does enable us to understand the process of realization. It appears to be unnecessary, therefore, to break entirely with epistemological methods in order to make valid the assumption of an independent outer-world.

<sup>1</sup> 'Die Realisierung,' p. 4.

But I bear no brief for realism on this occasion, however it shall be founded, and I have mentioned this issue which the new realists have raised merely to indicate that psychology does not make cogent solely such philosophies as the subjective idealism of a Berkeley, or the phenomenalistic positivism of a Mach. Furthermore, I wish to emphasize the fact, that the inadequacy of such idealistic contentions, far from arguing the limitations of psychology as a contributory basis for philosophy, only reveals the unsatisfactory character of the psychology which was thus applied. If philosophy would profit by psychology, the philosopher must overcome his cheerful acceptance of the naïve doctrines of sensationalism and association, as constituting the sole matters of importance in an adequate explanation of mind. The psychologist, as well, needs some enlightenment in this regard, and I know of no means by which he is more apt to secure it than by proposing to himself seriously the psychology which underlies logic.

The second and final problem which I wish to consider concerns the bearings of the thought-psychology upon the learning-process. This, I hope, will serve as a means of indicating the importance of investigations in pure psychology for determining the basic principles of behavior, of which the educator stands in such great need. I shall, however, consider this problem as it presents itself in the field of animal behavior.

In the lucid account of the learning-process presented in his volume entitled 'Animal Intelligence,'<sup>1</sup> Professor Thorndike states two provisional laws of acquired behavior, or learning:

"The Law of Effect is that: Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that, when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of the bond.

<sup>1</sup> P. 244.

"The Law of Exercise is that: Any response to a situation will, other things being equal, be more strongly connected with the situation in proportion to the number of times it has been connected with that situation and to the average vigor and duration of the connections."

While these laws are, as their author has explicitly stated, provisional, they nevertheless bear the authoritative approval of an eminent investigator in this field, and may, therefore, serve as a satisfactory point of departure in discussing this important problem of acquired behavior. The problem turns upon the condition or conditions which may be said to *fix* the selection of a certain process from among many which a situation calls forth in accordance with the purely automatic capacities of the organism. The fixing process of the law of effect is the accompanying or closely following "satisfaction," while for the law of exercise, it is repetition and the average vigor and duration of the connections. The truly selective factor is the satisfaction, for without this or some other motive there would be no means, save chance, for procuring the repetition of a particular behavior, or any special vigor or duration attaching to it.

But what, we may inquire, is the peculiar nature and efficacy of this feeling of satisfaction, and how is it induced? An animal which is confined in a puzzle-box is impelled to activity by the perception of food outside, and release, which is for the first time secured by a chance movement or combination of movements, may, indeed, be followed by a feeling of satisfaction while the food is being eaten. To connect this process of eating, however, with the preceding activity of release seems to require some retroactive effect of the feeling of satisfaction. Otherwise, it is difficult to understand how the satisfaction can be said to bind into associative union the action of release and the act of eating.

It may, of course, be argued that the "satisfaction" does not wait upon the actual achievement of the food, but sets in at once upon release. Yet this is not so easily maintained. The conditions of a sudden release are more nearly akin to an emotion of surprise, if we may judge by the analogy of human

experience, and one of the prime features of such a state is indecision, and a more or less complete disjunction with the impulse and aims of the previous situation. Even if it be granted that a feeling of satisfaction accompanies release, it is not at all clear how this satisfaction is able to *fix* the preceding movements and give them a special importance among the other movements which have been unsuccessful. It is questionable, too, if the mere addition of such an affective accent to a certain complex movement, and the attendant *perseverative tendency* which must be assumed to result, is sufficient ground for explaining the gradually more frequent selection of this movement by the animal until it becomes the sole response to the situation.

If we turn to the evidence adduced from the thought-processes for some further light upon the question, we may, I think, construct an alternate explanation which is more comprehensive, and which avoids the necessity of assuming a retroactive efficacy for affection, which, even in its more pronounced manifestations in adult human experience, is all too little understood. The most important finding of recent introspective research, and the one which has gained the widest acceptance among psychologists at large, is, I believe, the existence of *determining tendencies*. These tendencies are imageless "acts," to use the terminology of Külpe, which are revealed only *retrospectively* after a thinking-process has been completed. They are, nevertheless, *real* constituents of the process, for without their existence it appears impossible to give adequate explanation to a conscious action which aims at a definite result.

In the instance at hand, the situation of confinement and the attendant stimulus of the perception of food constitute the basis of such a tendency. This basis I should like to designate as a *nucleus*, by which I mean, primarily, a general physiological disposition. From this nucleus there issue various motor tendencies, and such reproductive ideational tendencies as the organism may be capable of. But the control exerted by the determining tendency is not explained by the sum-total of these responses. If it were, we should have no need for this special designation. The action of the organism would

be strictly mechanical in its nature, and learning would depend altogether upon chance collocations of events, thus falling entirely under the law of exercise as Thorndike states it. But such is not the true nature of animal behavior. Biologists as well as psychologists are coming more and more to the conviction that organic behavior cannot be adequately explained when the factor of purpose has been eliminated. And the determining tendency is precisely that which constitutes selection with a purpose.

It is evident that the operation of such determinations reduces the laws of association to secondary and purely mechanical effects. Recent research has brought abundant evidence to show that the contiguity and similarity of mental processes are an insufficient principle of explanation for organic behavior. In addition, there exists the purpose, always more or less defined, which exerts a monarchical supervision over the motor and ideational processes that are associatively called into being.

From this point of view learning becomes a process of defining the determining tendency, which is at first without adequate means of expression to realize its aim. The requisite expressive acts may be first hit upon by chance from among the many which the organism is capable of, but since the selecting principle exists *before* the act, rather than as an effect of the act, it is possible to fix in some manner, and stamp with a peculiar accent the successful performance *while it is in process*. The special application of the determining tendencies to this problem appears to have an advantage over such explanations as the one offered by Thorndike, inasmuch as the determining tendency operates *progressively*, whereas satisfaction can make itself effective only *retrogressively*.

This is as far as I am able to go in this matter with what seems to be of strict evidential value. But from studies of the determining effects upon thought-processes, I am inclined to carry the question a step farther in speculation. It appears to me that the *purpose* involved in the determining tendency may be regarded as a *conscious imageless* element which attends the formation of the nucleus, at first in a very vague and general manner, but later achieving greater precision, as trial and error

develop the possibilities, favorable and unfavorable, of response. This element may in turn be regarded as the basis of a thought-content which is distinct from sensory experience, and independent of it. While sensation and reproduced imagery may give precision and definiteness to thoughts, and are their natural mode of sequential expression, motor, verbal and imaginal, the thought arises from the nucleus, independent alike of specific sensory experience and memory. Not, to be sure, that thought has its existence apart from sensation, and uninfluenced by memory, but that the occasion for its appearance is not a memory image, but a physiological disposition which requires a conscious direction such as the sensory character of the situation does not afford. It is explained, therefore, by reference neither to sensation nor to memory. This, I believe, constitutes one of the most important grounds for rejecting the principle: *Nihil est in intellectu, quod non prius fuerit in sensu.*

Before leaving this problem of learning, it may be well to guard against the exception which may be taken to the theory here advanced on the ground that it is essentially *animistic*, and therefore rests upon a postulated purpose which many biologists are unwilling to assume. I must confess that my own leanings favor this assumption, and my presentation of the theory has doubtless been colored by this fact. It should, however, be clearly pointed out that the acceptance of the determining tendency as a principle of explanation for acquired behavior does not necessarily rest upon the further acceptance of animism. The determining tendencies, as originally discovered by Ach in the course of his investigation of volition,<sup>1</sup> were, indeed, found to issue from conscious purposes, but more recent results reported by Koffka on presentations in general, without special reference to volition,<sup>2</sup> have brought to light a sub-form of determination which he calls *latente Einstellungen*, and which we may perhaps translate rather loosely as latent adaptations. These are not voluntary, and do not issue directly from the primary purpose, but are rather of the nature

<sup>1</sup> Cf. 'Willenstätigkeit und Denken.' Göttingen, 1905.

<sup>2</sup> 'Zur Analyse der Vorstellungen und ihrer Gesetze.' Leipzig, Quelle und Meyer, 1912, p. 319 ff.

of chance methods which prove to be available. Frequently these are entirely unconscious, and indicate their existence only in certain type-forms of response. It is, therefore, conceivable that such tendencies as these might contribute to the formation of a habit without the assumption of a primary conscious purpose on the part of the animal.

I have, perhaps, said too much of these special problems. I fear that my interest in the methods and results of the new thought psychology may have seemed to place my main contention for a closer bond of union between psychology, on the one hand, and philosophy and education, on the other, upon too narrow a basis. I would not have it appear that the methods which seem to me so fruitful in effecting this union are the only ones applicable, nor that the hope of psychology to serve her sister disciplines rests upon the acceptance of precisely these methods and results. For, in spite of my personal faith in these, I realize only too well that the whole matter of the operations of thought is still in the controversial stage of its evolution. Accordingly, I can hardly flatter myself that I have been able to convince all that my results are adequate to my hopes. I trust, nevertheless, that some phases, at least, of this method of investigation may be approved as worthy of fair consideration, and that I may have succeeded in making plain the urgent need for some fundamental revisions in our scientific aims and theories, in order that we may be able in future to cope more effectively with those larger problems which the philosopher and the educator must, perchance, leave to the psychologist for solution.

My theme has been the close relationship which I believe exists and must continue to exist between psychology, philosophy and education. I have endeavored to show by reference to two special problems, the one bearing upon philosophy, the other upon education, that psychology is essentially a propædeutic to these two domains of knowledge. I rest my case here, then, with the hope that, despite differences of opinion regarding the value of the particular theories and results which I have made use of, the real demand for closer union between psychology and its two sister disciplines may have been set forth in a manner which will warrant of general assent.

## THE SENSORY THRESHOLD FOR FARADIC STIMULATION IN MAN<sup>1</sup>

BY E. G. MARTIN, E. L. PORTER,<sup>2</sup> AND L. B. NICE

A method of measuring induction shocks which has recently been developed by one of us<sup>2</sup> offers the advantage of enabling the investigator to take into account all the sources of variation which are present in induced currents as applied to tissues, including the electrical resistance of the tissues studied, and the manner of contact of the stimulating electrodes—factors not hitherto measurable.

This method we have employed in determining the human sensory threshold for induction shocks. Our results are interesting, both as affording a basis of comparison between the human sensory threshold and the threshold for various activities in the lower animals, and for their own sake, as criteria of the condition of the receptive mechanism of the human body. Earlier studies of the electro-cutaneous sensibility of the human body are those of Munk and Leyden,<sup>3</sup> Bernhardt,<sup>4</sup> and Tscheriew and de Watteville.<sup>5</sup> Their results, however, have little significance, in view of the relatively large sources of error inherent in their methods.

*Method of Measuring Stimuli.*—This method consists, in essence, of determinations of the strengths of faradic stimuli in terms of primary current intensity and secondary coil position, with corrections for the resistance of the secondary circuit and for the precise mode of contact of the stimulating electrodes with the tissue. The expression for stimulation strength uncorrected for these last two factors is  $Z = (M/L) \times I$ ,  $Z$  being the stimulation strength,  $M/L$  the "calibration

<sup>1</sup> From the Laboratory of Physiology in the Harvard Medical School.

<sup>2</sup> Martin, 'The Measurement of Induction Shocks.' New York, 1912; also the *American Journal of Physiology*, Vols. XXII., 1908, and XXVII., 1910.

<sup>3</sup> Munk and Leyden, *Virchow's Archiv*, 1864, XXXI., p. 1.

<sup>4</sup> Bernhardt, 'Die Sensibilitätsverhältnisse der Haut, Berlin, 1814.

<sup>5</sup> Tscheriew and de Watteville, *Brain*, 1897, II., p. 163.

number" for the particular secondary position used, and  $I$  the primary current in amperes. To correct for secondary resistance and for electrode variation the expression  $\beta = AZ/(R+A)$  is employed. The value  $\beta$  represents the effectiveness of the stimulus at the actual seat of stimulation, the physiological kathode or kathodes. Its validity rests upon the assumption that the resistance of these kathodes is negligibly small, since  $\beta$  signifies in actuality the amount of stimulus that would have to be used to obtain the observed result were the secondary resistance reduced to zero. For determining  $\beta$  a constant  $A$  must be established. This constant represents the influence upon the effectiveness of the stimulus of the mode of contact of the stimulating electrodes. To establish it two values,  $Z_R$  and  $Z_{R'}$ , must be determined for two secondary resistances,  $R$  and  $R'$ . The expression for  $A$  is  $(Z_R R' - Z_{R'} R)/(Z_{R'} - Z_R)$ .

*Stimulating Electrodes.*—At the outset of our investigation we were confronted with the problem of selecting and applying suitable stimulating electrodes. The first desideratum for satisfactory electrodes is that they shall minimize skin resistance. The horny layer of the skin with its oily secretions offers a relatively enormous resistance. We have obtained from a pair of platinum wire electrodes, pressed firmly against the finger tip, resistances ranging from 50,000 to 180,000 ohms. Obviously with such high initial resistances the opportunities for error are too manifold to permit one to have confidence in his results. A second essential feature of proper electrodes is that they shall maintain a perfectly uniform contact throughout the period of a single experiment. A third desirable feature is that the electrodes shall be as small as possible.

After experimenting with various sorts of electrodes we finally selected three types as meeting sufficiently one or all of the desiderata enumerated above to be worth using. Of these three one was to be applied to the lip and two to the fingers. The lip electrodes consisted simply of a pair of platinum wires bent over a small glass tube. By means of a harness fitting the head this glass tube was held so as to bring the platinum wires in firm contact with the mucous surface of the upper lip. These electrodes had the advantage of small stimulating

surface and also of maintaining a reasonably uniform contact between electrodes and tissues. Disadvantages presented by them were an undesirably high resistance, averaging above 30,000 ohms, and a possibility of being short circuited by the moisture of the lip.

Of the two types of finger electrodes one consisted of a pair of exceedingly fine steel needles soldered to flexible conductors and thrust horizontally about 2 mm. apart through the skin of the dorsal surface of a terminal phalanx. These electrodes satisfied the requirements of low resistance, small surface, and uniformity of contact. The only drawback to their use was a slight constant sensation of pain which sometimes became intense enough to interfere with accurate recognition of liminal shocks. The second type of finger electrodes consisted of a pair of small glass cylinders closed at their lower ends by corks through which platinum wires were inserted so as to extend a few millimeters into the cylinders. A drop of mercury covered each platinum wire, and the cylinders were filled above the mercury to within three centimeters of the top with strong sodium chloride solution. These electrodes were applied by flexing two fingers at the first joint and dipping their tips one into each cylinder to the depth of about two centimeters. Experiment showed that relatively considerable variations in the amount of immersion made no marked differences in the sensitiveness of the regions immersed. These electrodes proved the most satisfactory of those we used in respect to securing low resistance and strict uniformity of contact. Their large size introduced the objection that a great many receptors and much non-sensitive tissue were in the path of the shocks. The effect of this situation in actual practice will be discussed in later paragraphs.

*Method of Experimentation.*—In order that the subject of the experiment might not be distracted by the experimental procedures, wires were carried from the secondary coil of the calibrated inductorium through the wall into an adjoining room, where they terminated in connection with a pair of stimulating electrodes. A telegraph system connecting the two rooms enabled the subject and the experimenter to com-

municate with each other at pleasure. To avoid errors resulting from a possible diurnal variation in threshold each subject was experimented upon as nearly as possible at the same hour each day. The subject seated himself comfortably before a table on which rested a telegraph key and sounder, and adjusted the stimulating electrodes to lip or fingers as required. The experimenter, sitting before the stimulating apparatus in the next room, upon receiving notice that the subject was ready, began sending "break" shocks of calculable intensity at irregular intervals ranging from one to five seconds. The subject indicated by a telegraph signal whenever he felt a shock. The threshold was thus quickly determined, and by repeated tests shown to be the true one for the conditions then obtaining; next additional non-inductive resistances of twenty thousand, forty thousand and sixty thousand ohms, or in some experiments of forty thousand, seventy thousand and one hundred thousand ohms were successively introduced into the secondary circuit, and the threshold for each resistance determined in similar fashion. Readings for three additional resistances were taken instead of for just one, as required by the theory, in order to insure that serious errors should not creep in undiscovered. By thus obtaining four distinct thresholds at different resistances the calculation of four  $\beta$ 's for the same stimulus becomes possible, and a serious discrepancy among these  $\beta$ 's indicates at once that experimental errors have entered. Our practice has been to reject as untrustworthy any experiment in which three of the four calculated  $\beta$ 's failed to agree within ten per cent.

After the series of thresholds had been obtained, the secondary circuit, including the tissue being stimulated, but not the secondary coil, was connected with a Wheatstone bridge and its resistance determined by the Kohlrausch method. This resistance plus that of the secondary coil, which was known, and therefore not determined each time, gives the secondary resistance as a whole, and in connection with the data previously obtained permits the calculation of the specific sensory threshold for the particular conditions obtaining at the moment of the experiment. The whole

procedure was usually completed in about ten minutes. To illustrate the method of making the calculations the protocol of a single experiment is given below:

January 8, 1912. Subject M., experimenter P. Stimuli applied by means of wire electrodes on upper lip.

Secondary Resistance	Tissue Only —R— 23,000 Ohms	—R'— 63,000 Ohms	—R''— 93,000 Ohms	—R'''— 123,000 Ohms
Position of secondary coil...	17.7 cm.	14.7 cm.	13.8 cm.	13.2 cm.
Value of $\frac{M}{L}$ .....	381	986	1,400	1,790
Primary current in amperes —I—.....	0.5	0.5	0.5	0.5
Above corrected for core magnetization <sup>1</sup> .....	0.555	0.555	0.555	0.555
$Z = \frac{M}{L} \times I$ .....	$211 = Z_R$	$548 = Z_{R'}$	$777 = Z_{R''}$	$994 = Z_{R'''}$
$A$ , from formula $A = \frac{Z_R R' - Z_{R'} R}{Z_{R'} - Z_R}$ .....		2,080	3,100	3,960
Average $A$ .....	3,000			
$\beta = \frac{AZ}{R + A}$ .....	24.3	24.9	24.3	23.7
Average $\beta$ = .....	24.4			

*Validity of  $\beta$  as a Measure of the Sensory Threshold.*—If the value  $\beta$ , as determined by the method just described, is a true measure of the sensory threshold, we should be able to obtain closely concordant values of  $\beta$  in repeated experiments provided the sensory threshold of the subject has not varied meanwhile and notwithstanding variations in tissue resistance or in the method of applying the stimulating electrodes. As a test of the validity of our use of  $\beta$  in the manner indicated, we made five sets of experiments in duplicate on three different subjects, using the three types of electrodes described above. The interval between the first and second experiments of one set ranged from less than one half hour to more than two and one half hours; the five sets of experiments were carried out on five different days. These experiments, in our belief, demonstrate clearly the validity of the method adopted for measuring the sensory threshold, for in every one of them there was a close agreement between the two determinations

<sup>1</sup> For method of correcting for core magnetization see Martin, 'The Measurement of Induction Shocks,' p. 46.

of  $\beta$  although the resistance and the value of the constant  $A$  varied widely. The greatest difference in the values of  $\beta$  noted in any experiments amounted to 6.5 per cent. The average difference in all five was 3.5 per cent. The protocol of a representative one of these experiments is as follows:

February 16, 1912. Subject N., experimenter M. Stimulus applied to fingers through liquid electrodes.

1st observations: 2:30 P. M.  $Z$ , tissue only, 193; tissue resistance, 5,800 ohms; calculated  $A$ , 8,400;  $\beta$ , 104.

2d observations: 3:30 P. M.  $Z$ , tissue only, 265; tissue resistance, 8,300 ohms; calculated  $A$ , 5,800;  $\beta$ , 100.

As illustrating the superiority for measuring induction shocks, of  $\beta$ , in which secondary resistance is taken into account, over  $Z$ , in which secondary resistance is not considered, it is interesting to compare the values of  $Z$  given in these same five sets of experiments. The greatest difference between one  $Z$  and the other of a pair amounted to 27.2 per cent. The average in all five experiments was 15.3 per cent., differences more than four times as great as those noted above for the compared values of  $\beta$ .

*Comparison of the Human Sensory Threshold with the Threshold for Various Activities in Other Animals.*—When induction shocks are used as stimuli in ordinary animal experimentation, the stimulating electrodes can usually be placed advantageously with reference to the region to be studied. If the tissue stimulated is nerve the electrodes are in immediate contact with it, and carefully shielded to avoid escape of current to surrounding tissues. When muscle is stimulated directly the electrodes are so placed as to confine the current to the stimulated tissue. In experiments upon human subjects, on the other hand, the stimulating electrodes cannot be placed so advantageously. More or less escape of current is inevitable, and we cannot hope therefore to obtain human thresholds strictly comparable with those yielded in studies of lower animals.

The nearest approach in our experiments to the conditions obtaining in work upon experimental animals seemed to be in connection with the use of needle electrodes thrust through the skin. We had here, presumably, fairly intimate contact

of the electrodes with the receptors whose sensitiveness was under investigation. The electrodes were so near together as to make it probable that the greater part of the stimulating current passed directly from one to the other, with little escape to other tissues. Under these conditions we obtained values of  $\beta$  as follows:

Subject N. Average  $\beta$  (7 experiments) 14; subject P. Average  $\beta$  (3 experiments) 18; subject M. Average  $\beta$  (3 experiments) 14.5; subject C. Average  $\beta$  (2 experiments) 17.9. The average of all the above experiments was a  $\beta$  of 15.5. These tests were all made between two and three o'clock in the afternoon and, so far as possible, under uniform general conditions. No two tests on one subject were made on the same day.

In comparison with the above results on human subjects we have the following observations on experimental animals: Frog's gastrocnemius, stimulated through the sciatic (Martin, unpublished) average  $\beta$ , 20 cases, 0.84; frog's gastrocnemius, uncurarized, stimulated directly,<sup>1</sup> average  $\beta$ , 19 cases, 7.0; frog's gastrocnemius, curarized, stimulated directly, average  $\beta$ , 4 cases, 11.0; frog's sartorius, uncurarized, stimulated directly, average  $\beta$ , 4 cases, 7.0; frog's sartorius, curarized, stimulated directly, average  $\beta$ , 2 cases, 9.7. E. L. Porter<sup>2</sup> reports the following observations on cats: Threshold for wrist extension from stimulation of radial, average  $\beta$ , 14 cases, 1.4; threshold for reflex flexion of hind leg from stimulation of tibial, average  $\beta$ , 17 cases, 2.7. L. B. Nice in the course of a research not yet finished has obtained for the threshold of contraction of the diaphragm from stimulation of the phrenic in cats and rabbits an average  $\beta$  for 15 cases of 1.79.

That the results on man, reported above, represent probably a reasonably close approximation to the actual threshold for individual receptors, is indicated by the results of a series of experiments in which the platinum wire electrodes described on p. 195 were applied to the lip. In these experiments, although the tissue resistance was much higher than in the

<sup>1</sup> Published in part, 'Measurement of Induction Shocks,' p. 86.

<sup>2</sup> E. L. Porter, *American Journal of Physiology*, 1912, XXXI., p. 148.

experiments with needle electrodes thrust through the skin, averaging 31,000 ohms for the lip electrodes as compared with 10,000 ohms for the needles through the skin, the average  $\beta$  was virtually the same, 16 for the lip stimulation, as compared with 15.5 for the needles through the skin.

If we assume, as we probably may with justice, that the threshold for electrical stimulation of the tongue is substantially the same as that just reported for the lip, we may say in general terms, that a barely perceptible stimulus on lip or tongue is ten to twenty times as strong as the threshold for direct stimulation of the nerves of experimental animals, and twice as strong as the threshold for direct stimulation of frog's muscle.

*The Sensory Threshold an Index to the General Irritability of the Subject.*—In our experiments with electrodes of salt solution in which the fingers were dipped (p. 196) the conditions differed from those obtaining in our experiments with the other types of electrodes described, in two important particulars; the current was much more diffuse, and instead of being confined chiefly to the skin in the immediate neighborhood of the electrodes, was obliged to penetrate a considerable mass of underlying tissue. That the values of  $\beta$  given with these electrodes would differ widely from those yielded by the needle electrodes was to be anticipated. The liquid electrodes, have the advantage, however, of greatly lessened tissue resistance, and of less disturbing influence on the subject, in this latter respect proving far superior to the other types tested by us.

Our results with these electrodes are interesting for their uniformity. Early in our use of liquid electrodes it became evident that the values of  $\beta$  given by them in normal human subjects vary to a surprisingly moderate degree. This uniformity is dependent, of course, upon similarity of experimental conditions. Thus all readings were taken in the afternoon and whenever possible between two and three o'clock. The subject sat alone in a room, and concentrated his attention upon the fingers undergoing stimulation.

Our first series of experiments gave the following average

values of  $\beta$ : Subject P, average  $\beta$ , 13 experiments, 105; subject N, average  $\beta$ , 15 experiments, 113; subject M, average  $\beta$ , 15 experiments, 99.5. The general average of all these observations is a value of  $\beta$  of 106. With one of the subjects, P, the value of  $\beta$  from January 30 to February 8 did not drop below 100 and did not exceed 115.

Another series of observations was made upon a subject, L, through the kindness of Dr. F. G. Benedict, director of the nutrition laboratory of the Carnegie Institution of Washington. These readings were taken in the nutrition laboratory between 4:00 and 5:00 P. M. The average  $\beta$  of six readings was 120, the extremes were 112 and 130. A single reading upon another subject, O'C., taken at 4:00 P. M. gave a  $\beta$  of 97.

Mr. G. P. Grabfield in the course of acquiring mastery of the method for use in another research has made a number of single determinations of  $\beta$  on different individuals. He has kindly permitted the results of these determinations to be incorporated here. The values of  $\beta$  are 102, 72, 72, 110, 76, 119, 75, 82. Not all these readings were taken at the same time of day, and are not, therefore, to be compared so strictly as the results of our earlier series. They are significant, however, as showing that valid determinations can be made upon subjects who have not had previous experience in the use of the method, and also as additional evidence of the substantial uniformity of the sensory threshold when determined with liquid electrodes.

*Receptors Affected by Electrical Stimulation.*—A question of interest in connection with our studies of sensory electrical stimulation is as to the nature of the receptors affected by the electric shock. Thunberg, in his paper on the cutaneous senses, in Nagel's 'Handbuch der Physiologie' (1904, III., p. 698) includes electrical stimuli among those arousing pain. Our observations are in accord with Thunberg's classification, so far as concerns the method of stimulation in which needle electrodes are thrust under the skin. Our subjects were agreed that the only sensation given by electric shocks through these electrodes was pain, whether the stimuli were just at the threshold or considerably above it.

When wire electrodes pressing against lip or finger tip were used, there was equal agreement that stimuli at or just above the threshold aroused no pain sensation whatever, although stronger stimuli were undoubtedly painful. All our subjects described the threshold shocks as resembling touch. To test further the accuracy of this classification we arranged a pair of finger tip electrodes so that one of them could be moved slightly, altering the pressure against the finger. None of the subjects could distinguish any difference in quality between threshold electric shocks and the gentle pressure of the moving electrode.

Experiments with the liquid electrodes were performed on many more subjects than were experiments with the electrodes of the other types described. We made a practice of asking subjects to observe their sensations and to report them. Not a subject suggested that threshold shocks administered through liquid electrodes contained any painful quality. There was, naturally, considerable vagueness of description from those subjects who had had no experience in psychological experimentation. Fortunately several of our subjects had served formerly as subjects in psychological experiments. These gave such reports as that the sensation produced by the electric shock resembled the sensation which accompanies involuntary fluttering of the eyelid; or the sensation given when a finger is pulled till the joint cracks; or the sensation accompanying slight twitching of a muscle. From these reports, in view of the entire absence of definite conflicting observation, we consider it possible that the sensation aroused in this case is of muscle or joint sense.

A comparison of the methods of applying the stimuli affords ground for believing that the different types of electrodes may well stimulate different receptors. The current from the needle or wire electrodes is rather sharply localized, and would be expected to stimulate superficial receptors. The current from the liquid electrodes, on the other hand, is not only more diffuse, but also in its course penetrates more or less deeply the subcutaneous tissues, and might easily stimulate receptors of deep sensibility.

It is possible that the much higher threshold for stimuli applied through liquid electrodes, as compared with those sent in through needle or wire electrodes, is to be explained in part, at least, as due to the different receptors affected, and not wholly to the greater diffuseness of the current. Some incidental observations made in the course of this investigation lend support to such an idea. One of the subjects had on one of the days of experimentation a small cut on one of his fingers, and when that finger was used as one of those to be immersed in the electrodes the surprising result followed that the threshold was very much lower than the usual value. There was at the same time a marked change in the quality of sensation, each shock producing a distinct throb of pain, even at the threshold. At once after determining the threshold for the sore finger it was replaced by a sound finger and the threshold again determined. The painful quality was no longer manifest. The thresholds, in  $\beta$  units, were, for the sore finger 12; for the sound finger 88. To corroborate these observations another subject cut away, with a sharp scalpel, the superficial layers of skin from a small area of one finger, until great tenderness developed, but without bleeding. When stimuli applied through this finger were compared with those through sound fingers precisely similar observations to those reported above were made; there was pain felt in the sore finger at each stimulus, but not in the sound finger, and the threshold was much lower for the sore than for the sound finger. The figures for  $\beta$  were, in this case, respectively 44 and 105. Comparison of the values of  $\beta$  for sore fingers with the values for cutaneous stimulation given on page 201 show that they approach each other closely. In these experiments the general conditions were identical. Tissue resistance was unaffected by the small sore on the skin. The current must have been as diffuse in one case as in the other. A possible way to explain the result is by supposing the observed threshold with liquid electrodes to represent the true threshold for deep sensibility, and the relation of the cutaneous receptors to diffuse electrical stimulation such that they are unaffected by it unless through injury to the overlying protective skin they are made susceptible to it; in which case they respond at the threshold normal to them.

## SUMMARY

1. Determinations of human sensory thresholds for induction shocks were made with three forms of electrodes: *a*, wire electrodes pressed against the upper lip; *b*, needle electrodes thrust through the superficial layers of skin of the finger tip; *c*, liquid electrodes (sodium chloride solution) into which two fingers were dipped.

2. It was shown that a valid measure of the sensory threshold may be had in terms of  $\beta$  units; inasmuch as concordant values of  $\beta$  were given in repeated determinations of the sensory threshold notwithstanding wide variations in tissue resistance and in the contact of the stimulating electrodes.

3. When the stimulus was applied by means of needle electrodes thrust through the skin an average sensory threshold of  $15.5 \beta$  units was obtained. With wire electrodes pressed against the lip, the average threshold was  $16 \beta$  units. These values, compared with thresholds of  $0.84$  for frog's sciatic,  $1.4$  for cat's radial,  $1.79$  for mammalian phrenic,  $7$  for frog's muscle, uncurarized, and  $10-11$  for frog's muscle, curarized, suggest an electrical sensibility for the human skin fifty per cent. less than for frog's muscle, and one tenth to one twentieth that of naked nerve.

4. The human sensory threshold, measured with liquid electrodes, averages a value of  $100 \beta$  units, ranging between  $70$  and  $130$ . So constantly and in so many different subjects have thresholds fallen within this range that we conclude it to represent the normal for this method of stimulation.

5. The testimony of the subjects of these experiments as to the nature of their sensations under liminal electrical stimulation suggests that with needle or wire electrodes applied to the skin cutaneous receptors may be the ones affected; whereas with liquid electrodes the receptors to respond may be those of deep sensibility.

# PRELIMINARY REPORT OF A STUDY IN THE LEARNING PROCESS INVOLVING FEELING TONE, TRANSFERENCE AND INTERFERENCE

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## INTRODUCTION

This work was, primarily, motived by the desire to devise a set of experiments in the learning process that would be simple and economical in apparatus, that would involve conditions permitting many variations of minimal amount under ready control and that would make now greater, now less, demands, according to purpose, upon the sensory and motor processes, respectively. We hoped to profit by the example of the physicist and devise a seven-in-one apparatus which would serve to introduce and verify the more manifest laws of mind to large laboratory classes. For such purposes it seemed that situations creating concrete, cognitive and motor reactions, arousing decided feeling tones and the attitudes of attention and interest were desirable.

The more specific problems studied, the materials selected, the methods used and the results obtained form the basis of this report.

## SCOPE OF THE WORK

The facts observed and partially reported may be grouped about four topics: (1) The activities of the mind in learning, (2) the conditions and nature of transference and interference, (3) the correlation between the stages of learning and affective tones, (4) attitudes of attention and interest (omitted from this paper).

## MATERIALS

The materials employed were the distributing cases shown in the photographs, packs of ordinary playing cards, a stop-watch and a paste board screen. The cases contained fifty-

four compartments each, having six in the vertical dimension and nine in the horizontal. This arrangement furnished a compartment for each card of the pack of fifty-two, and at the same time preserved an approach to equality between the dimensions without a large excess of compartments. Each compartment was three and a half inches in length by two and three fourths inches in height, and admitted completely the average playing-card, being four inches in depth. These generous dimensions readily accommodated the cards, permitted a free unhampered distribution and obviated the necessity of overfine motor adjustment. The cases were stained dark brown in order to increase by contrast the prominence of the labels. Attention is again directed to the six by nine arrangement of the compartments which places twenty-six on the perimeter, thus creating conditions apparently more favorable to the study of the growth of the location sense than in the four by thirteen arrangement which places thirty on the perimeter.

The playing-cards were selected as suitable distributing material because of their cheapness, their accessibility, their standard nature and their universal and familiar character. This last mentioned quality tends to place those who approach the work upon common ground, in a position where fluency and acquaintance make for ease and freedom from constraint, thus taking from the task the formal and artificial tone so common where novel materials are employed.

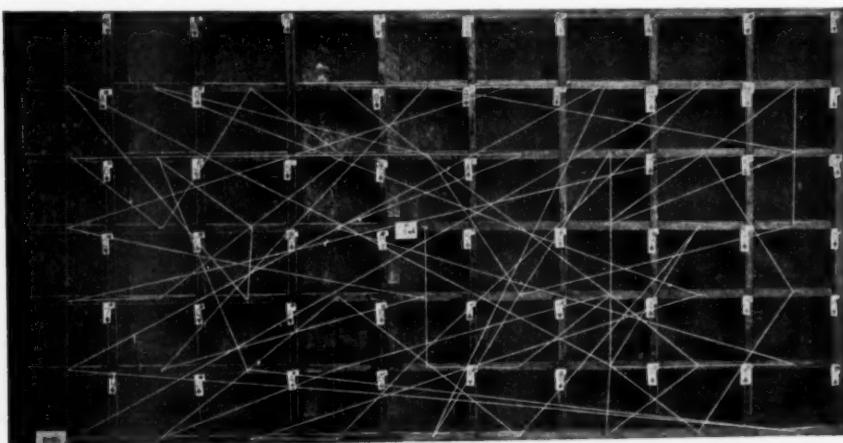
We note the earlier use of the playing-cards by Professor Jastrow<sup>1</sup> in 1886 when they were employed in determining the several forms of reaction time.

#### PLAN OF WORK

When the work was originally planned many of the developments were unforeseen. The intention was to label fifty-two of the fifty-four compartments permitting a distribution of one card to each, or as it has been considered, "by number, color and suit." Simultaneously with this piece of work another was to be pursued in which another subject was to distribute

<sup>1</sup> Jastrow, Joseph, *Science*, Vol. VIII., pp. 237-241, 1886.

"by number only" to the same case to a particularly designated suit. This subject, as will be seen, would use but thirteen compartments, throwing four cards of like number to a single compartment regardless of suit. To illustrate, if the compartments bearing spade labels were selected, the four queens would be thrown to the compartment bearing the label queen of spades, the same plan being followed with the other cards. Under this plan the problem of learning could be studied, progress being measured by the time required for distribution as taken with a stop watch, the interval measured being the time between the removal of the pasteboard screen previously mentioned and the call of "up" from the subject, indicating that the distribution was complete. It was planned to pursue this course until the possibilities of further improvement (as measured by the time interval) had been exhausted, when an exchange of work could be made by the two subjects,

CASE I.<sup>1</sup>

enabling a study of transference of skill and interference of the acquired habits.

The case was so labelled that the cards of a suit were not grouped, also that consecutive numbers did not appear upon

<sup>1</sup> Lines on case I. show direction of movements in distribution by *color, number and form*.

adjacent compartments. In order to keep all possible factors constant both for the daily tests and against the time of transference the cards were stacked and the same order used for both subjects. The order of the suits was spades, hearts, clubs, diamonds; and the numerical order of the cards was Q., 5, 6, 4, 3, 10, 8, 2, K., 9, 7, J., A.

It became evident that the case possessed other possibilities which seemed even more satisfactory than the plan already outlined. It was then decided to continue the above plan and in addition to undertake what follows. It appeared that if two subjects were to work upon different problems involving twenty-six compartments and two others upon two problems involving 39 compartments the tasks would be more nearly comparable than those involving thirteen and fifty-two compartments respectively, and that an exchange of work would mean improved opportunities to study transference and interference.

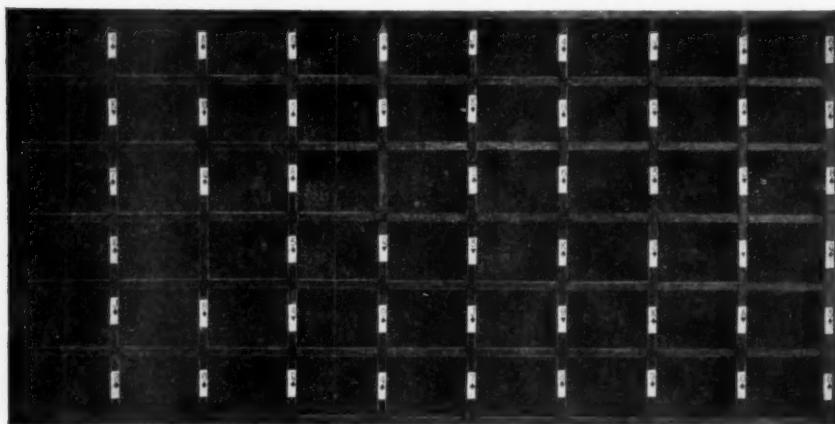
Accordingly, it was determined to have a third subject (paired with a fourth) distribute to 26 compartments according to the following plan: the red labels were to be used, and spades were to be thrown to hearts and clubs to diamonds. Hearts and diamonds were to be distributed to their own compartments respectively, or were to be 'resident.' The fourth subject was assigned the 26 compartments labelled spades and diamonds. The clubs were to be thrown to spades and the hearts to diamonds, the spades and diamonds being resident.

A third set of tasks was devised in which 39 compartments were utilized, the fifth subject using those labelled diamonds, clubs and spades. The fourth suit, hearts, was thrown to diamonds, the others were resident. The sixth subject, who exchanged with number five, used the same compartments but changed the plan of distribution by throwing clubs to diamonds and hearts to clubs. Diamonds and spades were then resident. There are then, six problems arranged in pairs of two each. And these pairs will be referred to as the 13-52 series, 26-26 series and 39-39 series respectively.

A second case No. 2 (see photograph) was provided for the last two subjects and the order of the suit for these two was

diamonds, clubs, spades, hearts, and the numerical order of the cards, 7, 4, Q., 2, 5, 9, J., 3, A., 10, 6, K., 8.

A "trial" consists of two distributions, the averages of which appear in the average time columns of Tables I. and II.; two distributions were allowed in order that the first might prepare for and yield facility for the second as a more accurate



CASE II.

measure of ability, the trials were given on alternate days. This plan was followed throughout, and so far as possible during the extended period over which the work was conducted, the same monitor served the same group of subjects.

Throughout the work, or until perfection had been attained, each subject made a weekly diagram of the case showing the location of only those compartments used in the distribution. The only opportunity afforded the subject for viewing the location of the compartments was during the period of distribution. These facts were gathered with a view to determine the nature of the relation existing between the rate of distribution and the growth of the position sense.

The subjects recorded their introspections, as far as possible at the close of the distribution. W. A. O. and L. W. K. are trained psychologists, I. B. a student of psychology and familiar with the demands of introspection, while K. P., S. J. O. and C. E. W. have had no psychological training.

## THE LEARNING PROCESS

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TABLE I.  
SHOWING PLAN OF WORK, DATES AND RATE OF DISTRIBUTION OF FIRST SERIES

I. R. Distributed by number only, ♀ resident		K. P. Distributed according to number, color and form		W. A. O. Distributed ♀ to ♂ and ♀ to ♀		C. E. W. Distributed ♀ to ♂ and ♀ resident ♀, ♂ and ♂ resident		S. J. O. Distributed ♀ to ♀, ♂ to ♂, ♀ and ♂ resident		L. W. K. Distributed ♀ to ♂, ♂ to ♀, ♂ and ♀ resident	
Date	Exp. No.	Exp. No.	Average time	Date	Exp. No.	Average time	Date	Exp. No.	Average time	Date	Exp. No.
Feb. 2	1	5'	43' 5	Feb. 2	1	6'	37' 5	Feb. 6	1	8'	44' 0
" 5	2	2'	38' 5	" 5	2	7'	7' 65	" 6	2	8'	48' 0
" 7	3	1'	33' 5	" 7	3	8'	23' 5	" 10	3	7'	34' 5
" 9	4	1'	2' 35	" 9	4	6'	10' 75	" 13	4	5'	49' 5
" 13	5	1'	3' 5	" 13	5	6'	2' 25	" 15	5	4'	44' 7
" 14	6	0'	51' 9	" 14	6	15'	6	" 16	6	4'	22' 1
" 16	7	0'	50' 75	" 16	7	5'	16' 5	" 17	7	4'	22' 7
" 19	8	0'	45' 3	" 19	8	5'	17' 75	" 20	8	2'	17' 25
" 21	9	0'	38' 25	" 21	9	5'	10' 4	" 22	8	2'	17' 25
" 23	10	0'	36' 4	" 23	10	4'	55' 5	" 24	9	2'	46' 2
" 26	11	0'	35' 72	" 26	11	4'	55' 4	" 27	10	2'	46' 2
" 28	12	0'	33'	" 28	12	3'	45' 65	" 29	11	2'	17' 75
Mar. 1	13	0'	36'	Mar. 1	13	2'	52' 75	Mar. 2	12	1'	40' 9
Distributed according to number only after interval of distribution as given in Table II		Average time		Average time		Average time		Average time		Average time	
Apr. 1	23	3'	37' 35	" 18	17	12'	30' 5	" 14	17	12'	20' 4
" 20	18	3'	36' 25	" 14	18	12'	23' 6	" 16	18	12'	28' 7
" 23	19	3'	36' 0	" 16	19	12'	19' 25	" 19	19	12'	33' 5
" 25	20	3'	16' 6	" 19	20	12'	19' 25	" 20	20	12'	25' 9
" 27	21	3'	16' 8	" 21	20	12'	8' 0	" 23	21	12'	35' 7
" 29	22	3'	7' 5	" 23	21	12'	12' 0	" 26	22	12'	22' 75
Apr. 1	23	3'	37' 75	" 26	23	12'	3' 4	" 28	23	12'	36' 4
" 29	2	2'	51' 25	" 28	24	12'	6' 3	" 30	24	12'	17' 1
May 1	3	0'	39' 35	" 3	25	12'	6' 3	Apr. 30	25	12'	18' 5
" 3	4	0'	33' 25	" 8	26	2'	59' 25	" 4	26	12'	14' 5
" 6	5	0'	31' 25	" 10	27	12'	11' 25	" 6	27	12'	27' 1
" 8	6	0'	31' 45	" 12	28	12'	38' 2	" 9	28	12'	14' 65
" 10	7	0'	29' 7	" 15	29	12'	38' 6	" 11	29	12'	9' 75
" 19	31	1'	32' 75	" 11	30	12'	55' 0	" 13	30	12'	14' 35
" 23	32	1'	32' 1	" 13	32	12'	51' 25	" 16	32	12'	19' 6
" 23	32	1'	32' 5	" 18	33	12'	46' 1	" 18	33	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 30	25	12'	18' 5	" 32	24	12'	35' 5
" 6	5	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 8	6	0'	31' 45	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 10	7	0'	29' 7	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 19	31	1'	32' 75	" 11	30	12'	55' 0	" 13	30	12'	19' 7
" 23	32	1'	32' 1	" 13	32	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	46' 1	" 18	33	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 6	5	0'	31' 25	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 8	6	0'	31' 45	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 10	7	0'	29' 7	" 17	30	12'	55' 0	" 19	30	12'	19' 7
" 19	31	1'	32' 75	" 11	30	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 1	" 13	32	12'	46' 1	" 18	33	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	51' 25	" 20	34	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 6	5	0'	31' 25	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 8	6	0'	31' 45	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 10	7	0'	29' 7	" 17	30	12'	55' 0	" 19	30	12'	19' 7
" 19	31	1'	32' 75	" 11	30	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 1	" 13	32	12'	46' 1	" 18	33	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	51' 25	" 20	34	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 6	5	0'	31' 25	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 8	6	0'	31' 45	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 10	7	0'	29' 7	" 17	30	12'	55' 0	" 19	30	12'	19' 7
" 19	31	1'	32' 75	" 11	30	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 1	" 13	32	12'	46' 1	" 18	33	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	51' 25	" 20	34	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 6	5	0'	31' 25	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 8	6	0'	31' 45	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 10	7	0'	29' 7	" 17	30	12'	55' 0	" 19	30	12'	19' 7
" 19	31	1'	32' 75	" 11	30	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 1	" 13	32	12'	46' 1	" 18	33	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	51' 25	" 20	34	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time		Average time		Average time		Average time		Average time		Average time	
Apr. 26	1	0'	42' 35	" 24	2	2'	51' 25	" 28	24	12'	17' 1
" 29	2	0'	39' 35	" 3	25	2'	11' 2	" 30	24	12'	23' 25
" 30	3	0'	33' 25	" 30	25	12'	18' 5	" 31	24	12'	24' 1
" 31	4	0'	31' 25	" 10	27	12'	11' 25	" 17	26	12'	17' 5
" 6	5	0'	31' 25	" 12	28	12'	38' 2	" 19	27	12'	20' 5
" 8	6	0'	31' 45	" 15	29	12'	38' 6	" 21	28	12'	14' 65
" 10	7	0'	29' 7	" 17	30	12'	55' 0	" 19	30	12'	19' 7
" 19	31	1'	32' 75	" 11	30	12'	51' 25	" 16	32	12'	17' 5
" 23	32	1'	32' 1	" 13	32	12'	46' 1	" 18	33	12'	17' 5
" 23	32	1'	32' 5	" 18	33	12'	51' 25	" 20	34	12'	17' 5
" 26	34	1'	32' 0	" 20	34	12'	51' 25	" 20	34	12'	17' 5
" 25	35	0'	47' 4	" 25	35	0'	47' 4	" 25	35	0'	47' 4
Average time											

TABLE II.  
SHOWING PLAN OF WORK, DATES AND RATES OF DISTRIBUTION OF SECOND SERIES

K. P.	I. B.	W. A. O.	C. E. W.			S. J. O.			Distributed $\diamond$ to $\diamond$ and $\diamond$ to $\diamond$ and $\diamond$ resident			Distributed $\diamond$ to $\diamond$ and $\diamond$ to $\diamond$ and $\diamond$ resident			Distributed $\diamond$ to $\diamond$ and $\diamond$ to $\diamond$ and $\diamond$ resident			L. W. K.					
			Date	Exp. No.	Avg. time	Date	Exp. No.	Avg. time	Date	Exp. No.	Avg. time	Date	Exp. No.	Avg. time	Date	Exp. No.	Avg. time						
Apr. 24	1	2'	30 <sup>0</sup> .25	Mar. 5	1	6'	29 <sup>0</sup>	Apr. 27	1	5'	21 <sup>0</sup> .3	Apr. 28	1	4'	33 <sup>0</sup> .5	June 19	1	1'	58 <sup>0</sup> .6	June 18	1	3'	45 <sup>0</sup> .4
" 26	2	2'	15 <sup>0</sup> .4	" 6	2	5'	22 <sup>0</sup> .5	" 30	2	3'	46 <sup>0</sup> .9	" 25	2	3'	29 <sup>0</sup> .1	" 21	2	1'	24 <sup>0</sup> .	" 20	2	1'	52 <sup>0</sup> .
" 29	3	3'	49 <sup>0</sup> .4	" 8	3	3'	54 <sup>0</sup> .75	May 2	3	3'	9 <sup>0</sup> .75	" 27	3	4'	46 <sup>0</sup> .6	" 22	3	1'	8 <sup>0</sup> .45	" 22	3	1'	50 <sup>0</sup> .45
May 1	4	4'	13 <sup>0</sup> .7	" 11	4	3'	45 <sup>0</sup> .35	" 4	4	2'	33 <sup>0</sup> .6	" 29	4	4'	46 <sup>0</sup> .6	" 26	4	1'	3 <sup>0</sup> .	" 25	4	1'	30 <sup>0</sup> .5
" 3	5	5'	8 <sup>0</sup> .5	" 13	5	3'	53 <sup>0</sup> .65	" 7	5	1'	56 <sup>0</sup> .75	May 2	5	1'	44 <sup>0</sup> .4	" 28	5	1'	58 <sup>0</sup> .	" 27	5	1'	32 <sup>0</sup> .4
" 6	6	6'	53 <sup>0</sup> .4	" 15	6	3'	17 <sup>0</sup> .4	" 9	6	1'	55 <sup>0</sup> .2	" 4	6	1'	44 <sup>0</sup> .4	" 28	6	1'	51 <sup>0</sup> .	" 29	6	1'	22 <sup>0</sup> .5
" 8	7	7'	44 <sup>0</sup> .75	" 18	7	2'	55 <sup>0</sup> .9	" 11	7	1'	40 <sup>0</sup> .4	" 7	7	1'	18 <sup>0</sup> .	" 1	7	1'	57 <sup>0</sup> .	July 2	7	1'	33 <sup>0</sup> .
" 10	8	8'	46 <sup>0</sup> .	" 20	8	2'	25 <sup>0</sup> .25	" 14	8	1'	45 <sup>0</sup> .5	" 9	8	1'	14 <sup>0</sup> .	" 1	8	0'	58 <sup>0</sup> .	" 4	8	0'	28 <sup>0</sup> .
" 22	9	9'	57 <sup>0</sup> .5	" 22	9	2'	57 <sup>0</sup> .5	" 16	9	1'	35 <sup>0</sup> .5	" 11	9	1'	13 <sup>0</sup> .	" 8	9	0'	57 <sup>0</sup> .	" 6	9	0'	27 <sup>0</sup> .
" 25	10	10'	44 <sup>0</sup> .25	" 25	10	1'	44 <sup>0</sup> .25	" 18	10	1'	25 <sup>0</sup> .55	" 14	10	1'	16 <sup>0</sup> .	" 10	10	0'	54 <sup>0</sup> .	" 9	10	0'	23 <sup>0</sup> .
" 27	11	11'	22 <sup>0</sup> .	" 27	11	1'	22 <sup>0</sup> .	" 21	11	1'	18 <sup>0</sup> .75	" 11	11	1'	10 <sup>0</sup> .	" 12	11	0'	53 <sup>0</sup> .	" 11	11	0'	17 <sup>0</sup> .
" 29	12	12'	8 <sup>0</sup> .5	" 29	12	1'	8 <sup>0</sup> .5	" 23	12	1'	18 <sup>0</sup> .9	" 14	12	1'	8 <sup>0</sup> .	" 15	12	0'	49 <sup>0</sup> .	" 16	12	1'	14 <sup>0</sup> .
Apr.	1	13	1'	37 <sup>0</sup> .75	" 13	1'	25	" 13	1'	17 <sup>0</sup> .4	" 25	13	1'	8 <sup>0</sup> .	" 17	12	0'	53 <sup>0</sup> .	" 18	13	1'	15 <sup>0</sup> .	
" 3	14	14'	59 <sup>0</sup> .1	" 14	14'	1'	29	" 14	14'	17 <sup>0</sup> .4	" 28 <sup>0</sup> .5	" 24	14	1'	58 <sup>0</sup> .	" 19	14	0'	49 <sup>0</sup> .	" 20	14	0'	12 <sup>0</sup> .
" 5	15	15'	0'	" 15	15'	0'	59 <sup>0</sup> .1	" 30	15	1'	43 <sup>0</sup> .75	" 25	15	1'	57 <sup>0</sup> .	" 22	15	0'	49 <sup>0</sup> .	" 21	15	1'	8 <sup>0</sup> .
" 8	16	16'	1'	" 16	16'	1'	17 <sup>0</sup> .3	June 1	16	1'	13 <sup>0</sup> .75	" 28	16	1'	57 <sup>0</sup> .	" 23	16	0'	47 <sup>0</sup> .	" 22	16	1'	7 <sup>0</sup> .
" 10	17	17'	0'	" 17	17'	0'	55 <sup>0</sup> .25	" 6	17	1'	18 <sup>0</sup> .2	" 29	17	1'	3 <sup>0</sup> .	" 31	18	1'	7 <sup>0</sup> .	" 23	17	1'	7 <sup>0</sup> .
" 12	18	18'	0'	" 12	18	0'	51 <sup>0</sup> .95	" 8	18	1'	16 <sup>0</sup> .5	" 31	18	1'	7 <sup>0</sup> .	" 31	18	1'	45	" 31	18	1'	45
" 15	19	19'	0'	" 15	19	0'	48 <sup>0</sup> .95	" 11	19	1'	10 <sup>0</sup> .75	June 8	19	1'	0'	" 17	18	1'	45	" 18	18	1'	45
" 17	20	20'	0'	" 17	20	0'	48 <sup>0</sup> .85	" 13	20	1'	16 <sup>0</sup> .5	" 10	20	1'	0'	" 19	19	1'	65	" 19	19	1'	65
" 19	21	21'	0'	" 19	21	0'	46 <sup>0</sup> .9	" 15	21	1'	17 <sup>0</sup> .4	" 7	21	1'	0'	" 20	20	1'	57 <sup>0</sup> .	" 20	20	1'	57 <sup>0</sup> .
" 22	22	22'	0'	" 22	22	0'	49 <sup>0</sup> .9	" 18	22	1'	8 <sup>0</sup> .	" 35	18	1'	0'	" 12	12	1'	59 <sup>0</sup> .	" 12	12	1'	59 <sup>0</sup> .
" 24	23	23'	0'	" 22	47 <sup>0</sup> .	5	47 <sup>0</sup> .	" 20	23	1'	5 <sup>0</sup> .	" 32	20	1'	0'	" 14	14	1'	57 <sup>0</sup> .	" 14	14	1'	57 <sup>0</sup> .
" 27	28	28'	0'	" 27	28	0'	58 <sup>0</sup> .	" 21	28	1'	17 <sup>0</sup> .4	" 35	21	1'	0'	" 17	17	1'	59 <sup>0</sup> .	" 17	17	1'	59 <sup>0</sup> .
" 30	29	29'	0'	" 30	29	0'	54 <sup>0</sup> .	" 29	29	0'	58 <sup>0</sup> .	" 4	29	0'	0'	" 1	29	0'	49 <sup>0</sup> .	" 1	29	0'	49 <sup>0</sup> .
" 33	31	31'	0'	" 31	30	0'	53 <sup>0</sup> .	" 30	30	0'	57 <sup>0</sup> .	" 4	30	0'	0'	" 3	30	0'	49 <sup>0</sup> .	" 3	30	0'	49 <sup>0</sup> .
" 36	32	32'	0'	" 32	31	0'	51 <sup>0</sup> .	" 31	32	0'	51 <sup>0</sup> .	" 1	31	0'	0'	" 8	32	0'	46 <sup>0</sup> .	" 8	32	0'	46 <sup>0</sup> .
" 39	33	33'	0'	" 33	32	0'	49 <sup>0</sup> .	" 32	33	0'	53 <sup>0</sup> .	" 5	32	0'	0'	" 10	33	0'	50 <sup>0</sup> .	" 10	33	0'	50 <sup>0</sup> .
" 42	34	34'	0'	" 34	33	0'	53 <sup>0</sup> .	" 33	34	0'	49 <sup>0</sup> .	" 4	33	0'	0'	" 12	34	0'	53 <sup>0</sup> .	" 12	34	0'	53 <sup>0</sup> .
" 45	35	35'	0'	" 35	34	0'	49 <sup>0</sup> .	" 34	35	0'	58 <sup>0</sup> .	" 4	34	0'	0'	" 15	35	0'	51 <sup>0</sup> .	" 15	35	0'	51 <sup>0</sup> .
" 48	37	37'	0'	" 37	36	0'	50 <sup>0</sup> .	" 36	37	0'	54 <sup>0</sup> .	" 4	36	0'	0'	" 17	36	0'	46 <sup>0</sup> .	" 17	36	0'	46 <sup>0</sup> .
" 51	38	38'	0'	" 38	37	0'	54 <sup>0</sup> .	" 37	38	0'	54 <sup>0</sup> .	" 4	37	0'	0'	" 19	37	0'	43 <sup>0</sup> .	" 19	37	0'	43 <sup>0</sup> .
" 54	39	39'	0'	" 39	38	0'	47 <sup>0</sup> .	" 38	39	0'	47 <sup>0</sup> .	" 4	38	0'	0'	" 22	38	0'	42 <sup>0</sup> .	" 22	38	0'	42 <sup>0</sup> .
" 57	40	40'	0'	" 40	39	0'	47 <sup>0</sup> .	" 39	40	0'	47 <sup>0</sup> .	" 4	39	0'	0'	" 24	39	0'	44 <sup>0</sup> .	" 24	39	0'	44 <sup>0</sup> .
" 60	43	43'	0'	" 43	42	0'	47 <sup>0</sup> .	" 42	43	0'	47 <sup>0</sup> .	" 4	42	0'	0'	" 26	40	0'	43 <sup>0</sup> .	" 26	40	0'	43 <sup>0</sup> .

## THE LEARNING PROCESS

The problem of distribution, viewed as a learning process reveals upon the conscious plane the following distinct phases in order of appearance: (1) That of forming connections between the cards and their respective labels, or briefly 'establishing card,' (2) that of learning the location of the compartments within the case, (3) that of learning the sequence of the stacked cards and (4) that of establishing a synthesis between the known compartments and the familiar order of the cards. These phases will be considered in turn.

As a part of the learning process in both series 'establishing card' means changing by means of mental substitution, association or otherwise the suit of the card to correspond to the label of the compartments; and, in general, to mentally alter the suits of the pack as required by the plans of distribution to match the labels involved.

Two methods were used: (1) Wherever suit and label differed, the name of the suit was changed to that of the label; (2) or the plan or scheme of distribution was repeated. Concerning the first method C. E. W., February 10, says (the dates given with every introspection correspond to those in Tables I. and II.): "In placing the black cards in the red boxes I immediately upon sight of the card to be placed change its name in my mind *e. g.*, I at once call a spade a heart." W. A. O., February 3, says: "I kept repeating the name of the suit to which I was required to bring the other suit of the same color, *e. g.*, on the appearance of 8 of clubs I found myself repeating '8 of spades,' '8 of spades.' Then when the 8 of spades really came I was lost for the moment, thinking it necessary to reverse its form, which of course was not necessary this time. This method for making sure of the standard suit for the color was employed, *viz.*, repetition of the number in my hand and the standard suit for the color." The introspections of L. W. K., February 17, show use of the second method: "Repeating the conditions of distribution aids me (hearts to diamonds, spades, clubs and diamonds resident). The repetition was oral." February 20: "I rehearse the conditions mentally as I distribute." February 24:

"Repeating the conditions for the cards of low number is still an aid." The cards of low number gave all subjects more trouble than the face cards and those of high number. L. W. K., March 9, makes this record: "I have better control of the diamond holes than of the clubs and spades." This indicates that the subject had previously 'established card,' and had now shifted attention to the location of the compartments. Both of these methods led ultimately to the same goal in which the character of the card was absorbed, so to speak, into that of the label, became in meaning, as far as it involved action, identical with the label. The growth from the highly conscious stage up to the sensori-motor and on to the point in which individual cards are lost to consciousness and disposed of automatically as a minute part of a large movement, can not be detailed for the lack of evidence. The introspections show, however, that verbal repetition very soon gives way to silent recall and this process merges into a sensori-motor form of action. W. A. O., February 6: "Everything is coming to be interpreted as a spade or a diamond." February 8: "The association of hearts to diamonds and clubs to spades is becoming quite automatic, though I think the verbal expression has much to do in guiding my search and identifying the appropriate compartment. *Feel quite sure now of perhaps 8 or 9 of the locations.*" February 10: "There is no thought that a red card is anything but a diamond, or a black card anything but a spade. In looking at the card in my hand I note the number and the color. Red is firmly associated with diamonds and the stronger pulse of attention is given to the number." S. J. O., 1st series, February 23: "In all my work I have had to repeat the label of clubs compartment (diamonds) while a club was in my hand, *e. g.*, repeated for 8 of clubs, '8 of diamonds, 8 of diamonds.'" March 1: "Did not feel it quite so necessary to repeat, in crossing the cards, the denomination of the hole into which I was placing them." March 6: "I do not *feel* towards the colors as I did at first. It may be because I know better where the different cards belong."

These few examples may serve to illustrate the growth up to the sensori-motor stage, and further to show that at this

stage (1) a few locations are determined and (2) the run of the cards begins to dawn. It appears that these two sources of knowledge pave the way for the established card to become automatic. The conditions of the 13-52 series rendered establishing card practically negligible. The exchange of work in the other series introduced many elements of interference which greatly increased the difficulties of establishing card; the introspections of three of the subjects, S. J. O., L. W. K., and W. A. O., show that they never reached the automatic stage for a majority of the cards. But so far as the learning progressed it followed the course of the first series as described above.

Opportunity for viewing the location of the compartments, as before stated, was possible only during the exposure of the case and in actual distribution. It is evident, too, that in the matter of number of compartments to be determined the series (13-52, 39-39, 26-26) differ inherently in difficulty. The facts of the growth of location of compartments were obtained from introspections and from a study of the charts of the case. The charts were made weekly, as previously mentioned, from memory and when arranged in a time series revealed the growth of the sense of position. The weekly charts are combined in Table III. under four captions, viz., Rt. (right), 1 R. (one remove), 2 R. (two removes) and "scattered." The latter refers to those placed more than two compartments from the proper location. The growth in the case of each subject was from general to exact locality or from "scattered" through the intermediate steps to "right" (see Table III.). The records of K. P., C. E. W., and L. W. K. do not indicate exactly growth as it occurred, for at the outset only those compartments were charted which were felt to be known, making it necessary in compiling Table III. to class those uncharted as "scattered." It is highly probable that the growing "feel" for locality would have brought many now listed as "scattered" within the two removes or one remove class. This growth from general to exact locality is revealed in the introspective accounts. L. W. K. on March 19 writes with regard to two compartments previously uncertain: "The

TABLE III.  
SHOWING GROWTH OF KNOWLEDGE OF POSITION OF COMPARTMENTS IN THE CASE AS MEASURED BY ABILITY TO PLACE THEM IN A MAP

I. B.				K. P.				W. A. O.				C. E. W.				S. J. O.				L. W. K.						
Distributed by number only. 13 compartments				Distributed according to number, color and form. 32 compartments				Distributed by color and number. resident. 26 compartments				Distributed by number and special color. resident. 26 compartments				Distributed by number, special color and special form. resident. 39 compartments				Distributed by number, special color and special form. resident. 39 compartments						
Date	Rt	1R	2R	Date	Rt	1R	2R	Date	Rt	1R	2R	Date	Rt	1R	2R	Date	Rt	1R	2R	Date	Rt	1R	2R			
Feb. 3	6	5	0	Feb. 7	4	0	48	Feb. 10	4	1	20	Feb. 26	5	4	1	29	Feb. 18	0	1	1	36					
" 10	1	2	0	" 16	8	4	6	" 10	7	0	7	" 1	3	19	3	1	1	5	" 24	3	3	1	30			
" 16	9	2	0	" 23	11	1R	0	" 20	14	9	3	" 11	13	3	2	21	Mar.	2	6	6	1	26				
" 23	13	0	0	" 14	0	2	36	" 24	10	13	3	" 14	16	14	10	" 9	11	10	3	15	" 15					
				" 14	0	2	36	" 24	10	13	3	" 15	16	14	10	" 2	16	15	13	1	10					
				" 13	2	3	34	" 2	15	10	1	" 22	23	13	1	" 22	29	10	0	0	" 23	16	0	7		
				" 13	2	3	34	" 11	14	10	2	" 16	24	0	2	" 16	22	11	3	4	" 30	19	13	5		
				" 16	4	0	32	" 17	16	10	2	" 23	26	0	0	" 13	32	7	0	0	" 14	6	28	1		
				" 20	24	3	3	" 17	20	6	0	" 20	38	11	0	" 13	28	9	2	0	" 20	30	8	0		
				" 22	22	2	26	" 23	22	4	0	" 29	33	6	0	" 27	26	12	1	0	" 27	26	12	0		
				" 22	28	5	0	" 30	26	0	0	" 29	6	27	12	" 10	32	7	0	0	" 13	35	4	0		
				" 27	5	2	18					" 17	31	8	0	" 17	31	8	0	0	" 18	35	4	0		
												" 25	30	9	0	" 25	30	9	0	0	" 25	39	0	0		
Mar.				Scattered				Scattered				Scattered				Scattered				Scattered						
" 13	19	10	1					" 13	19	10	1	" 21	21	13	0	" 10	32	7	0	" 10	35	4	0			
" 15	20	12	0					" 15	20	12	0	" 21	21	13	0	" 11	30	9	0	" 11	35	4	0			
" 22	35	7	3					" 22	35	7	3	" 21	21	13	0	" 12	30	9	0	" 12	35	4	0			
" 25	39	8	1					" 25	39	8	1	" 21	21	13	0	" 13	30	9	0	" 13	35	4	0			
" 29	41	5	6					" 29	41	5	6	" 21	21	13	0	" 14	29	23	2	" 14	35	4	0			
" 3	52	0	0					" 3	52	0	0	" 9	26	0	0	" 5	26	0	0	" 5	35	4	0			

Rt = Located correctly.

1R = One remove.

2R = Two remove.

Scattered = Greater than two remove.

second distribution profited in an unusual degree by the first. Neighborhood locations gave way to exact ones." This statement is repeated in substance throughout the accounts of the other subjects. The introspections converge further upon the following points: (1) The compartments on the perimeter of the case are the first to be fixed; compartments in and about the center are the most difficult; the cards of greater significance in familiar card games, as ace, king, queen, etc., are more promptly and easily located; on the other hand cards of low number are refractory. C. E. W., March 5, says: "The two spot of diamonds still worries me. Every time I come upon the miserable little thing it throws me into a panic." This subject had previously commented on the same card at three previous distributions. (2) Where a change of plan brings new compartments into the problem, the old serve as points of reference for the new. (3) Compartments are readily located during actual distribution, but are quite difficult and often impossible to locate when they are to be charted. S. J. O., March 1: "Have a feeling of the location of a majority of the compartments, but find it difficult with the chart before me to label them correctly." It is as though the exact knowledge requisite to chart a performance were of no utility in the functioning habit. All the evidence tends to show that imagery of both compartments and movements rendered but slight if any direct aid to the rate of distribution; the latter waited upon the automatization of the processes involved. Later it will be seen that imagery aided in anticipatory and unifying functions.

It is not until attention may be diverted from the locating of the compartments that the subjects find the learning of the order of the cards becoming a conscious process. It is not necessary that the compartments be correctly charted but that there may be enough habit in delivering a card to permit attention to turn from the fixing of location. At this time sufficient distributions have been made to favor recognition of a system or regularity in the cards and to constitute beginnings upon the serial learning involved. A detailed account of the manner in which W. A. O. learned the sequence of the

cards is given here. On February 17 cards are reported as known: at the beginning of the pack, Q. of S., 5 of D.; toward the end 10 of S., 8 of D., and at the end A. of D. Introspections for the same date state, "The 8 of D. is known to follow the 10 of S. because this order coincides with the sequence of compartments to which these cards are thrown." On February 22, the order is thus reported, "Q. of S., 5 of S, . . . , 10 of S., 8 of H, . . . , J. of S., A. of D." On February 24 the following order: "Q. of S., 5 of H., 4 of D., K. of S., . . . , 10 of S., 8 of H., . . . , J. of S., A. of D." with this introspection. "Feel a dawning of the order in the cards and believe that I shall know them soon."

On February 27, the following order: "Q. of S., 5 of H., 6 of C., 4 of D., 3 of S., . . . , J. of S., A. of D." with this introspection "the order seems more and more natural, even though I cannot enumerate the cards; so natural, in fact, that I had the thought as I placed them up, 'I surely know this part of the run.' When I now tried to name them, however, it was impossible." This is also noted: "I recognized the 10 of S. followed by the 8 of H., and the 10 of C., followed by the 8 of D., and there always comes a sense of sureness and familiarity at this point."

On February 29 this observation is made: "Have a feeling that the cards are running hearts, clubs, diamonds, spades, but this is due to my conviction about the first few and the last two rather than to observance throughout the task. Think that the 2 of S. follows the 8 of D. which succeeds the ten of C., but this is part inference and part memory."

On March 2 he speaks of feeling satisfaction at finding "Q. of C., 5 of D., 6 of S., 4 of H., in the center of the pack, different form, it is true, from the first run as I have inferred now while writing it out, but which I did not notice in passing because I do not distinguish between clubs and spades, hearts and diamonds." The failure to distinguish between clubs and spades, hearts and diamonds adds to the difficulty of serial learning in this plan. They are not distinguished because a club is a spade and a heart is a diamond as already explained in connection with the subject 'establishing card.' The

account closes "Thought I knew which card follows the 3 of S., but find it very uncertain now. Think it is the 10 of H." The significance of these introspections as related to the real order becomes apparent by referring to the order given in the plan of work. [See p. 209.]

The introspective account for March 7 is included entire because it explains how the mind resolved the 52 cards into four sequences of thirteen cards each, like as to sequence of number but varying as to sequence of suit and grasped the relation between them—a step made by all who mastered the run, and a possibility entirely unforeseen at the outset, when learning the order signified learning a unitary run of 52.

On March 7 at the close of the distribution the following order is given: *He ignores the suit*: Q., 5, 6, 4, 3, 10, 7, 9, J., A., with the introspection below twelve hours later: "The run of the cards as indicated above was written immediately after the test. Since that time I have straightened out the run of the cards. It came about in this way: I had a period of wakefulness in the night, and immediately upon awakening my thoughts turned to the cards. I am apt to be especially clear as to memory at such a time. Immediately after running over Q., 5, 6, 4, 3, 10 which was the red, black series starting with the black, I recalled (just a chance association it seemed, though I have no doubt habit entered in) that I have known for some time that a red 8 followed a black 10—then like a flash, 'Why not a black 8 after a red 10?' I suspect the 8 came from habit and I merely corroborated it with that mental comment." That is, he was reasoning from the known order in parts of the second run (2nd 13) to the unknown order in a similar part of the first run; for he adds: "Starting with a black queen (1st run), must demand a black 8 after a red 10. Then it occurred to me that I had noticed this before, but never had been sufficiently impressed to recall it at the time of writing.

"The rational process would not let the matter alone then, so I went through what I knew, supplied the missing links as best I could, and wrote down the following upon getting up."

S.	H.	C.	D.	S.	H.	C.	D.	S.	H.	C.	D.	S.
Q.	5	6	4	3	10	8	K.	2	7	9	J.	A.
H.	C.	D.	S.	H.	C.	D.	S.	H.	C.	D.	S.	H.
Q.	5	6	4	3	10	8	K.	2	7	9	J.	A.
C.	D.	S.	H.	C.	D.	S.	H.	C.	D.	S.	H.	C.
Q.	5	6	4	3	10	8	K.	2	7	9	J.	A.
D.	S.	H.	C.	D.	S.	H.	C.	D.	S.	H.	C.	D.
Q.	5	6	4	3	10	8	K.	2	7	9	J.	A.

On March 9 this note regarding order appears: "Am confident now of the run of the cards, and find that my previous impression needs two alterations, the 2 precedes the King and the 9 the 7." These changes establish the correct order. Barring the variations due to individuality the account given above is typical of the manner in which the run of the cards was learned by the other subjects except I. B. It appears that serial learning is accomplished by the mastery of certain groups which establish themselves at the beginning, at the end and about some conspicuous card within the sequence; it may be a face card, a card conspicuous as to location or a combination of these. The growth then is by a budding process, the extension of old groups or the addition of new, until the intervening gaps are filled and the sequence is complete.

I. B. interpreted the order of the cards in terms of the sequence of compartments used in distribution. February 9: "Although I am sure I could not read off the arrangement of the cards when away from the box, when in actual distribution I find myself looking for the next place, and correctly; and I find myself calling off the cards as they appear or *rather the name of the box that I need to put the next card in*" (Italics the writers'). February 13: "I notice that the cards were grouped and felt that there must be four groups." On February 16 the subject writes out correctly the numerical order of the groups, and on February 19 this introspection: "When I first began to work, as I located a box for which I was looking it seemed to stand out from the others like the main object in a picture, but as I grew more familiar with the places this

distinctness faded out and now I pay very little attention to the boxes at all, my whole concern is with the groups. I found myself putting groups one and two away (first and second thirteens) without realizing it. When I found myself on group three I was a little startled and was not sure whether I had put the others in their right places. The first of every group is accented, and the ace of the last group means 'up,' not 'ace.'

I. B.'s method is but a natural result of the problem presented in the 13-52 series. The thirteen compartments were promptly learned and as they repeated four times in each distribution it constituted a means of analyzing the fifty-two cards into four sequences—a process much more laboriously learned by the other subjects—which were then learned individually.

The fourth step on the conscious level viewed as a learning process is that of synthesis between the known compartments and the acquired order of the cards. By this is meant the attaching of the order of the cards to the compartments moved over in the distribution, so that *order* signifies order of compartments, and compartments cease to be localized as individuals, lose their identity as such, and are treated as factors in unified groups composed of several compartments and symbolized by a single image. The process is a slow one, accomplished only, according to introspective evidence, by repeated distribution and not appreciably aided by reflective repetition of the performance nor by knowledge of the order of the cards as such. The earliest evidence of assistance from memory of the order of the cards and of position of boxes (judged by the time standard) is found in the ability of the subject to *anticipate* the next compartment or succession of compartments permitting increased speed over that segment of the distribution. S. J. O., May 17: "I seem to know exactly where each card belongs and to anticipate the approaching card and its location so that no time is lost in thinking out the proper compartment." L. W. K., April 11: "I discovered the beginning of the synthesis between the positions and the run of the cards." April 16: "I feel now that any rapid gain

will be due to anticipation and instant recognition of location. There were no memory lapses, but here and there a false motion or a nervous jerky movement from box to box even though I knew what to do. I believe I can smooth these out by long reaches of *anticipation*." June 11 (second series): "The ability to anticipate the next card, and in some instances six cards, *e. g.*, 7 of S., 4 of C., Q. of D., 2 of H., 5 of S. and 9 of C., is now an automatic process." The subject conceived the movement necessary to deliver these six cards as a "fancy letter 'Z.'" W. A. O., April 6: "Think the factor of anticipation an important one in the elimination of the gaps which exist between segments of the distribution. I put it in those places because . . . it enables the delivery to pass smoothly between the segments or over the *nodes*." He further states: "There are times when I throw automatic runs so rapidly that it absorbs all my attention (the mere mechanical placing and grasping of the cards do this), and I reach the less automatic territory (containing compartments latest exactly localized) entirely without preimagination of what is next. It is here that time is lost." It would then appear to be the function of anticipation to unite these segmental automatic sections into a unified whole. There is also something which functions as effectually as anticipation within the segments and in a sense stands for and feels like it, and yet it is more of a complacent attitude of at-homeness in the situation, a confidence that you will not go wrong, such as one may have on a well-known stairway when he stretches out his foot with confidence and without anything which could be called conscious anticipation. This indicates that a later stage than that in which certain segments of the distribution are brought under one image, in which the synthesis of card-sequence and compartments is perfect, is that stage in which not only individual images of compartments are lost but also those later symbols representing unified groups of compartments which have been variously described in the introspective accounts as a 'fish hook swoop,' a 'Jacob's ladder' of crossing and recrossing, and a 'variety of polygonal figures.' It would seem that the possibilities of the problem in the directions of speed and perfection of place-

ment would only be reached when the synthesis enabled the entire distribution to be included within several segments, imaged at first, but later so thoroughly knitted into a perfect automatism that imagery is negligible, if present at all. And we will then have come upon a fifth stage in the learning process. It would be instructive just here, did space permit, to check the resemblances and differences between these five stages of learning and those recorded by Cleveland in his account of 'The Psychology of Chess and of Learning to Play It.'<sup>1</sup>

#### TRANSFERENCE AND INTERFERENCE: OBJECTIVE

For descriptive purposes the facts of transference and interference are grouped into two classes, the objective and the subjective. The former involves: (1) permanency or change of compartment, (2) a card retaining or changing in the second series the label to which it is thrown and (3) motion toward a compartment from the same or a different direction. These three rubrics express the kind and amount of possible objective transference and interference involved in the change of work from the first to the second series. These facts were determined by a study of the respective pairs of distributions as shown in the 'maps of distribution,' and the results are here tabulated in Table IV. The facts listed and numbered in columns 'same' classify as transferable and those in columns 'different' under 'compartment' and 'direction,' respectively, as interferable. And the column headed 'same' under 'Card' means that a card was thrown to the same label as in series I. (transferable), while the column headed 'different' means that a card was thrown to a different label in series II. (interferable). Furthermore the facts tabulated in the columns 'compartment' and 'Card' made a direct appeal to visual perception in the distributing process and are thereby considered receptive or sensory facts while those appearing in column 'direction' required kinaesthetic or motor responses and are reckoned as expressive or motor facts. Their meaning is partially found in the form of the learning curves, in the kind and number of

<sup>1</sup> Cleveland, A. A., 'The Psychology of Chess and of Learning to Play It,' *Amer. Jour. Psychology*, July, 1907, Vol. 18, pp. 293-296.

TABLE IV.  
SHOWING ELEMENTS OF TRANSFERENCE AND INTERFERENCE BETWEEN I AND II SERIES FOR EACH OF THE 52 CARDS IN TERMS OF COMPARTMENTS,  
CARD AND DIRECTION

8 = Same.

**d = Different.**

errors and more explicitly in the subjective groups of facts submitted below.

TABLE V.

SHOWING THE NUMBER OF SENSORY AND MOTOR UNITS OF WORK FOR TRANSFERENCE AND INTERFERENCE RESPECTIVELY

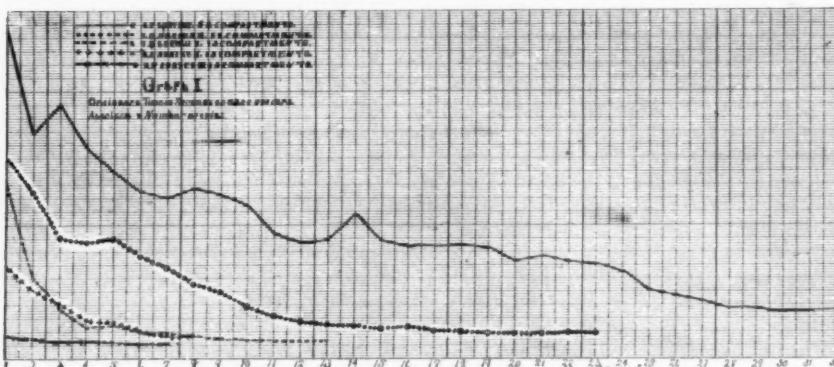
Series	Transference				Interference				Grand Total
	Sensory	Per Cents	Motor	Per Cents	Sensory	Per Cents	Motor	Per Cents	
13-52	26	16.66	12	7.68	78	49.99	40	25.64	156
26-26	26	16.66	3	1.92	78	49.99	49	31.41	156
39-39	52	33.33	10	6.41	52	33.33	42	26.92	156

Table V. is constructed from the totals of Table IV.

A summary of these transferable and interferable units shows: (1) that the 26-26 series plan produced the greatest amount of interference and afforded the least amount of transference, (2) that the 13-52 series gave the next highest amount of interference and next to the least amount of transference, and (3) that the subjects performing the 39-39 series encountered the least interference and enjoyed the greatest transference. This numerical estimate of the units of work finds corroboration and expression in the curves of graphs I., II., and III.<sup>1</sup> The curves for the second series of graph II. show a comparatively small deviation from the curves of first series. W. A. O.'s second series curve, save for differences of individuality and possible transference, should coincide with C. E. W.'s first series curve, being expressions of similar work; the fact that it does not so coincide presumes—barring differences of individuality—that the extent of its deviations is a measure of the amount of transference. The deviations decrease rapidly so that the curves well nigh coincide at the 23d trial and actually do so at the 30th and 33d trials. Equally, if not more instructive is the small deviation of C. E. W.'s second series curve from that of W. A. O.'s first series. They show a uniform convergence up to the 21st trial, and from there to the 35th, or end, maintain a close coincidence which is interpreted as a minimum of trans-

<sup>1</sup> The ordinate indicates the time in seconds required to place a single card and the abscissa the number of the trial.

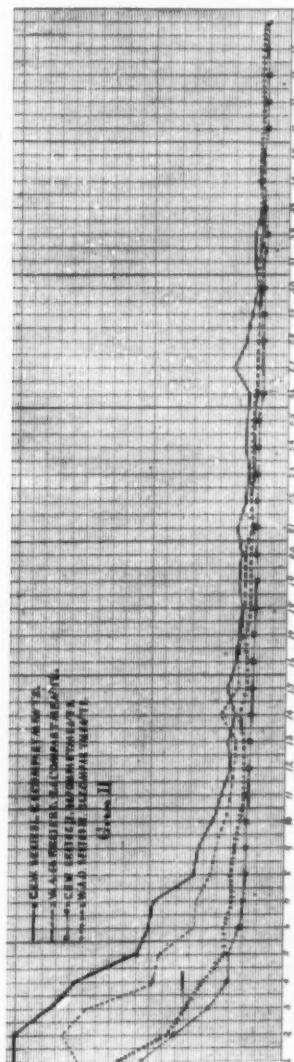
ference. I. B.'s curve, second series, and K. P.'s first series, graph I., show a larger deviation in rate than the similar curves of graph II. The marked difference in the individuality of the subjects and the absence of further facts leave the amount of transference undetermined. K. P.'s curve, second series, shows but slight deviations from I. B.'s curve, first series, and while transfer is in evidence the difference in individuality and



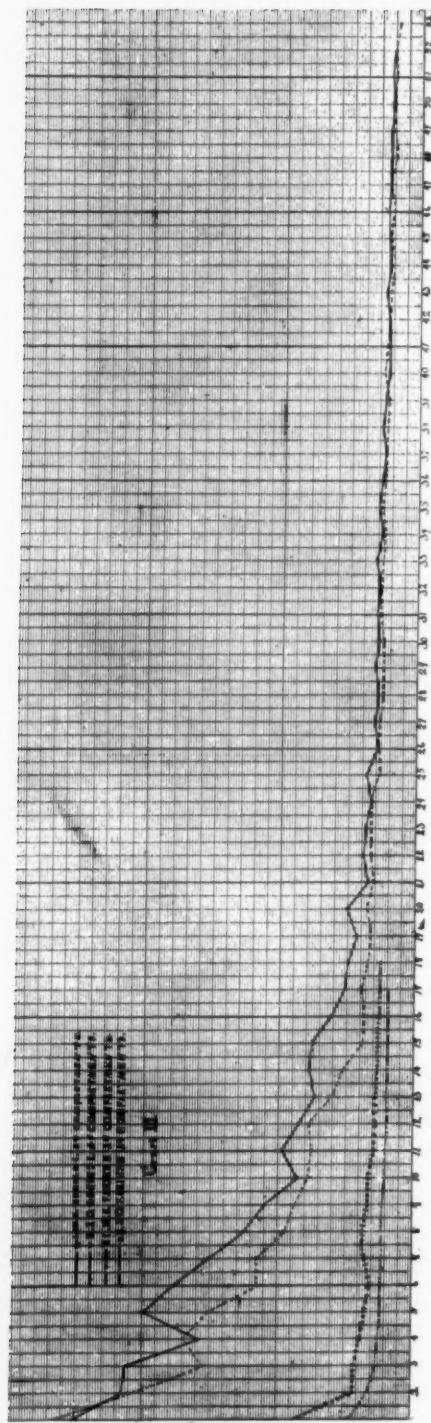
the comparative simplicity of the 13-52 distribution place the amount of transfer in doubt. The curves of graph III. show a consistent deviation in toto between the curves of the 1st and 2d series and in their first parts exceed the deviations of those of graph I., which checks well with the largest amount of possible transferable units.

It appears also that the nature of these units, *i. e.*, as to whether they are sensory or motor, has a decided influence on their value for transference and interference. The ratio of the transferable and interferable sensory units of work for both the 13-52, 26-26 series is one to three (1/3) and for the 39-39 series it is one to one (see Table V.); the motor units show the ratios of one to three and three tenths (1/3.3), of one to four and two tenths (1/4.2), and of one to sixteen and three tenths (1/16.3) in favor of interference for the 13-52, 39-39 and the 26-26 series respectively. Reference to Tables I. and II. and to graphs I., II., and III. shows that for similar work in the second series of distribution a minimum rate of increase over the first series was made by the 26-26 series, a

medium rate by the 39-39 series and a maximum by the 13-52 series. Now these several relative rates of increase correlate inversely with the amount of motor interference: the



ratio of motor interference in order of relative amounts to transference being as indicated above  $1/3.3$  (13-52),  $1/4.2$  (39-39) and  $1/16.3$  (26-26); and the relative rates of increase



in an ascending series by plans of work being 26-26, 39-39 and 13-52. These facts seem to indicate that a *disturbance of the motor elements* involved in the act of learning is a more serious handicap than a somewhat comparable *disturbance of the sensory elements*.

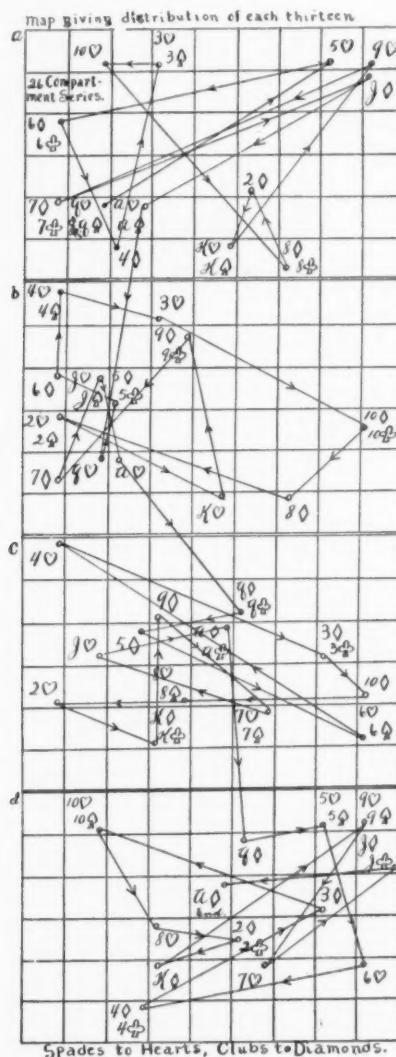
#### INTERFERENCE AND TRANSFERENCE: SUBJECTIVE

Interference means here an inhibition in the distribution of the cards and in learning by reason of responses to a former series; and transference implies an increase in the distribution and in the learning rate over a normal one on account of responses to a former series. The conditions of interference as well as transference for the subjective aspect are of three sorts similar to the objective: (1) change in compartment, (2) change in card in relation to label, (3) change in direction both to and from the compartments. The subjective results of these conditions are partially given in the introspections here submitted.

1. *Change in Compartments*.—I. B., March 5: "I wanted to put all cards where I had previously put them." March 6: "I have not yet discovered any sort of arrangement of the cards." (The cards were stacked as in her first series; that is, she has lost the order of the cards.) W. A. O., April 27: "Found the old system interfering and drawing my hand here and there despite my intention to reflect before placing the cards." C. E. W., May 16 (2d series): "I started out without giving any attention to what I was to do, thinking that it would take care of itself. After a few throws I became confused as to whether I was to throw the red cards into the heart or diamond boxes and found it difficult to determine which it was." W. A. O., May 4, 2d series: "Found the K. of H. going to the K. of D. place automatically and the Q. of S. moved against judgment toward the lower left hand corner" (see photograph No. 1). "The run of the cards is unconscious, the hearts difficult to find, and they still possess a definite tendency to be placed with the diamonds as in my old plan."

2. *Change of 'Card,' or Label*.—In the 26-26 series a club

thrown to diamonds instead of spades is change of card only, since diamonds were 'resident' in both series. W. A. O., April 30: "I found the consciousness necessary to place a club

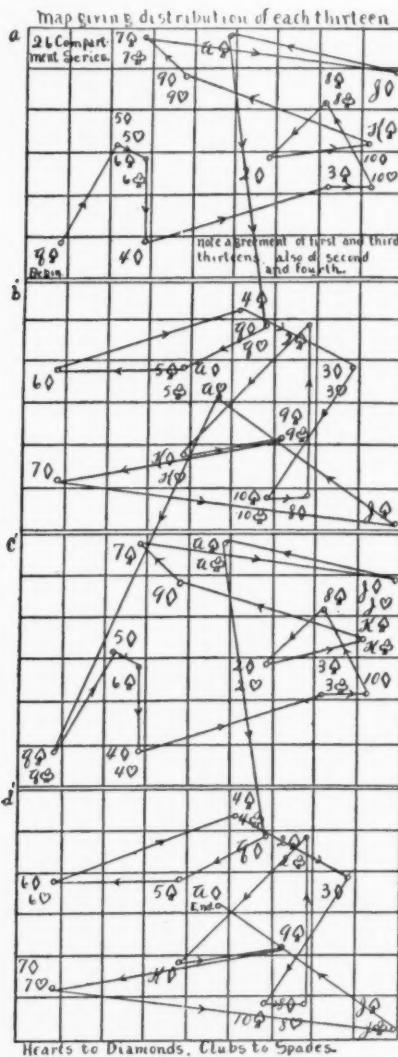


with a diamond disturbed me in finding the familiar diamond places; for example, the three of clubs became such a conscious matter through the need of thinking diamonds continually

(aided by repeating 'diamonds, diamonds') that I was unable to go directly to the well-known three of diamonds place, and the sight of a diamond did not release the response as it normally would have done (because I have thrown the diamonds right along) owing to my feeling that I must deliberate before making *any* throw." L. W. K., June 18: 2d series: "Compartments for fifty deliveries were instantly recalled and located when wanted, time was lost in determining what I wanted; I had to think hearts as diamonds in the first series, now it consists in thinking clubs as diamonds and hearts as clubs. My problem is one of substitution in the perceptual field." Of course this perceptual aspect of interference, while readily and early apparent, is partial and constitutes interference only to the extent of the activity of the contrary perceptual associations formed in the preceding series. And the new associations as such classify simply as a learning process. A more serious aspect of interference involving both that of 'card' and 'direction' is indicated in the following introspection: L. W. K., June 22: "Although there are no new boxes to learn, the old boxes being retained, I do not feel as confident of their location during the distribution as I did at the first and second trials in the new series. Taking a *different* card by *new route* to an old box is breaking down the former habits of approach to the box and as a consequence has weakened my memory for position. This weakened and temporary memory lapse occurs during the distributing process with the *card in my hand*. As soon as the performance is over I can locate any box called for without hesitation. The location of the boxes, and the run of the cards are well known *separately*, but neither are well known when *taken together*. When I try to adapt the new plan to the box I lose both the 'run' and the locations."

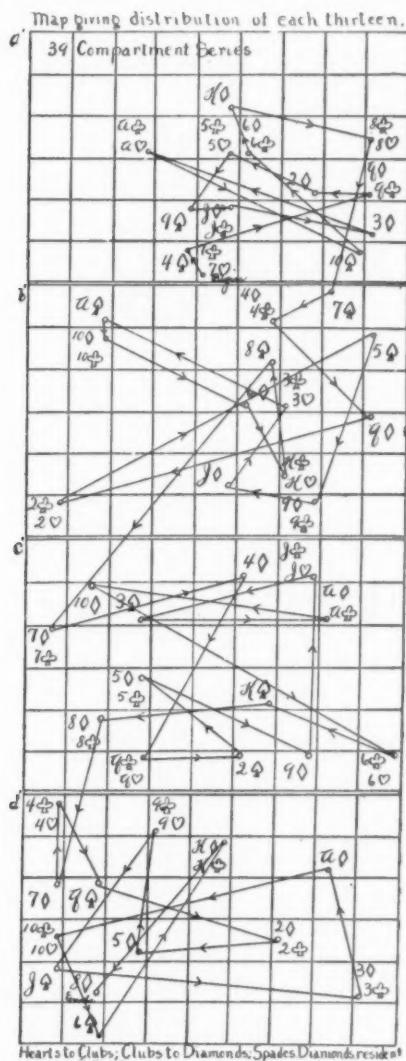
S. J. O., June 19, 2d series: "I started this new series just as I did the preceding. In order to "fix" the "cross" of hearts to diamonds, with each heart card I would repeat the word "diamonds" until the card was placed in its proper location. It was somewhat difficult to prevent habit from throwing clubs to diamonds instead of to clubs. Just the mere thought that the run was new set up a confusion which thwarted every attempt at speed."

3. *Change of Direction.*—Change in direction is a result of change in box and in 'card,' and, as interference, differs vitally from the other two conditions in that it involves the motor



aspect of the distributing process. S. J. O., July 5: "The 7 of C. bothers me in each run. I start to place it in the box for the 7 of D. (see photograph No. 2) showing that the habit

acquired in the first series has not yet been broken." The reverse interference, *i. e.*, a tendency to throw clubs to clubs occurred with L. W. K. July 4: "There were brief lapses in



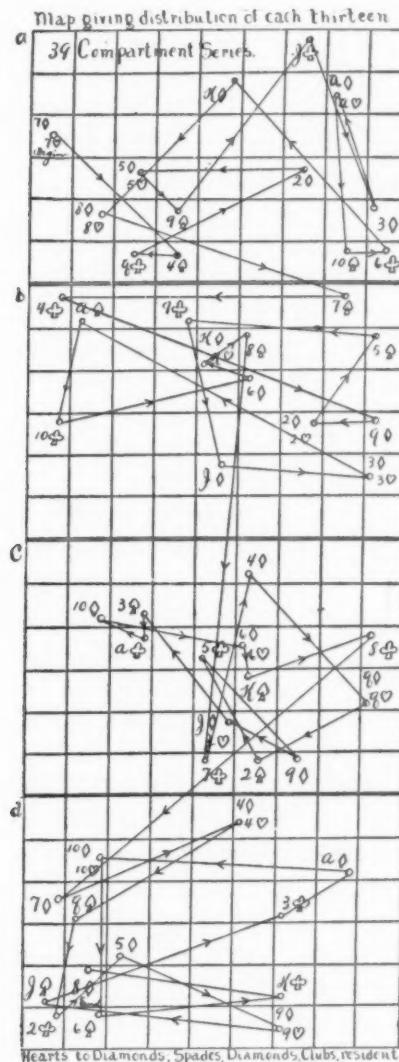
my distribution today. The 10 of C. to 10 of D. This is due to imperfect association of clubs to diamonds and the inhibition of the former association. Decided hesitation occurs with

the 7 of C., 9 of C. and with the 7, 9, and 10 of hearts (the latter, hearts, formerly went to diamonds, now to clubs). When these cards are reached in the pack there is a momentary suspense until the box is recalled. I can feel the right movement 'coming' somewhat similar to the experience of recalling a name or place." W. A. O., May 11: "I feel perfectly at home with reference to the location of the boxes. I observe that two heart boxes are called for and then two diamond in succession—or two new, then two old, but the old ones do not have the same sequence as in the first series, and while I know the run of the cards and am able to anticipate, the new sequences of boxes hinder speed."

The interference, thus far described may be considered direct in that it consists of habituated and contrary responses to box, card and direction. Other and less direct forms appear and vanish with the progress in the stages of learning. One of the first to appear is a memory lapse due to changes in antecedent movements. L. W. K., June 27: "The memory of 8 of S. appears to be due to the new distribution of K. of H. to K. of C. instead of the K. of D., as in the former series, and at once followed by 8 of S. The new order broke up the old habit so that when the 8 of S. appeared, it felt strange and unknown for several seconds" (see distribution maps, 39 compartment series, sections *b'* and *b*). The extent of the possible effect of *new* antecedent movements upon the distribution to permanent compartments may be determined objectively by a comparative study of the distribution maps section by section. Another form of interference to appear as soon as the subject is able to anticipate is that of confusion caused by throwing a card to the next box ahead in the sequence. It may be termed an ideo-motor error as it does not belong to interference proper. All subjects who reach the anticipatory stage experience confusion from this source.

*Errors.*—The cause of errors were of four sorts: interference 43.75 per cent., mechanism 83.75 per cent., confusion 12.5 per cent., and emotion 25 per cent. The errors of interference have already been indicated and those of emotion will be described under a separate heading. An error of mechanism consists

in throwing a card to a compartment one or two removed due to an over or underreach and is neither connected with an apparent conscious process nor an interfering habit. The few



errors of confusion consisted in mistaking the figure 3 for an 8 or a 5, and a 6 for a 9 on cards of similar color, and in throwing cards of like numbers and of similar color together. This

latter error was not strictly confusion but rather sensori-motor attraction in which the number rather than the suit dominated attention.

*Transference* means the responses, ideas and experiences of the former series that favor speed and the learning process in the second series. I. B., March 5: "The old practice aided in that I knew the places for the thirteen spades." March 6: "All of the spades are like old friends, they settle into their former places so easily and comfortably." March 15: "Some of the places I locate with reference to the spades." In the thirteen series, number mediated directly between card and box. I. B. learned the thirteen series first, and the prominence played here by number seems to have made it a means to learning the 52 series. I. B., March 27: "I find myself becoming more conscious of the cards than of the boxes; and I say the number of the next card to come and think of it as a number, not as a kind, *e. g.*, when the 8 of H. is to come, I say the number 8 and place it where it belongs." I. B., April 1: "I think my first work (meaning first series) fixed the run of the cards and now I am just joining to this the position of the card without reference to its color and form." W. A. O., May 11: "Used the run of the cards today for the first time, and instead of saying 'heart, heart, diamond, diamond,' I said Q., 5, 6, 4, 3, 10, 8, 2, etc." The ability to use transferable elements is conditioned by the facility to perform and the degree of mastery over certain parts of the problem, *e. g.*, L. W. K. could not use the 'runs' until the new sequences of boxes were partially mastered, and likewise W. A. O., not until the new boxes had been well established in sequence. And, further, grouping and symbolizing awaits a partial synthesis between location and 'runs.' The idea of grouping and symbolizing on account of favorable relations between sequences of the 'run' and the contiguity of the boxes was discovered in the first series and applied as soon as possible in the distribution of the second series. And to this may be added that of giving special attention to the 'ends' and 'beginnings' of runs; *i. e.*, the 13's since both cards and the boxes at these points served as landmarks toward and from which useful groupings were established.

## MOTOR ABILITY AND AFFECTIVE PROCESSES

The distributing situation created conditions that produced a wide range of affective tones and gave direct evidence on the formation of a number of the more common attitudes, feelings and even emotions.

*Bewilderment.*—K. P., February 5: "Was conscious of bewilderment at first as to any definite place, tried to fix certain positions I noticed while looking for others. Found this useless at least consciously so." S. J. O., March 22: "The jack of clubs gave me trouble in locating and I looked for it until I was bewildered by the sea of boxes before me. I felt as if the indicator for that box must have fallen off." C. E. W., February 8: "I noticed that at times the characters designating the boxes became so blurred that I could not distinguish them at all for an instant. I am unable as yet to say whether this is due merely to a blur of colors and forms in passing rapidly over the characters or whether it is not due to an extensive loss of general consciousness." The latter cause is probably correct, due to strained and prolonged attention. This evidence coming at the beginning of the work appears to show that a dazed and bewildered attitude arises from a sense of helplessness and from a total lack of means and methods of procedure.

*Confusion.*—S. J. O., March 27: "A sort of fog seemed present throughout the trial which lifted only as I reached the last ten in the deck. I found myself giving expression to my confusion by moving the card to be placed around in a circle." K. P., February 9: "Sometimes when I feel that a box is in a certain place, I find it is not, and this causes such confusion of mind that the next few cards are harder to locate." C. E. W., February 6: "Sometimes lost time by thinking I remembered the location of the box when I did not. Would experience a sense of being 'rattled' which occasioned loss of speed." L. W. K., February 22: "Placing the third card in the series consumed so much time that it checked my speed in placing four successive cards." Confusion may occur with any rate of progress and from a variety of conditions. One of its marked characteristics is its diffusiveness.

*Distrust and Timidity.*—K. P., February 16: "I relied too much on preconceived localizations. They are likely to be erroneous. This made me distrust my ability to find boxes. Lost time three times in this way." Several cases of this sort. S. J. O., March 29: "I seemed to flounder about a good deal, feeling that a card was in one place when in many cases it was in the very opposite direction." C. E. W., February 24: "Certain cards still confuse me every time I come to them. I can explain it only by the fact that I have come to consider them with apprehension and therefore when they appear it throws me into a sort of 'mental panic.'" L. W. K., March 5: "The attitudes of distrust and confidence are becoming well marked off from each other. Distrust accompanies a complete loss of even 'neighborhood' location of the box, a feeling that I have never seen the location and that it would do but little good to see it. This gives rise to aimless wandering. This 'wandering' experience occurred four times in today's distribution and it not only consumed time but impaired confidence in well determined locations. The distrust borders on fear, when I anticipate its repetition with those cards farther on in the pack whose locations are still doubtful." Distrust seems to arise from the realization of false imagery especially of a motor sort.

*Anxiety and Dread.*—I. B., April 3: "The effort to keep from making a mistake seemed to lengthen the time." L. W. K., March 26: "I delivered the cards in both distributions by sheer knowledge, rather than by a smooth-running habit. Distrust, hesitation and superfluous movements accompanied both distributions. I worked in dread that some entirely unlocated card would appear. The 9 of spades answered to this dread, 3 of spades also ominous." S. J. O., April 22: "I fumbled two cards and the feeling that I was not doing well enough made me somewhat vexed." L. W. K., March 7: "While the entire situation is less strained, and a more confident attitude prevails, there is at times a feeling of fear or distrust that I will experience an utter lapse with reference to some card or cards farther on in the pack. Have I here the genesis of a phobia based on the experience with certain

intractable cards? Some of these troublesome cards are more easily visualized than those whose boxes are known. I even see these cards when engaged in other work."

*Humiliation.*—W. A. O., March 5: "Seemed very much at sea from outset. The box was newly painted, was turned so I faced the door. Miss B. entered the room and engaged the monitor in conversation. My emotional tone was markedly unpleasant, there was nothing enlivening, actuating, inspiring about it. It was the heavy dull emotion akin to sullenness and hopeless loss. It seemed to effectually clog the channels of expression, and I went on doggedly, filled with chagrin and certain that all effort was useless." S. J. O., May 1: "The 7 of D. played me such a miserable trick—left consciousness entirely for a few seconds and confused me so much that I did not overcome the feeling for the rest of the run, and was very much ashamed when I finished to have gone back so badly." (Subject means that the time of 2d distribution had greatly increased beyond the normal.) L. W. K., February 27: "I felt considerable confidence in the start and the first four cards went rapidly, the five of D. was a serious puzzle and completely balked me, and when I realized that I had fallen behind my last record a feeling like despair came over me. I felt 'finished.'"

*Hesitation.*—S. J. O., April 19: "During the second distribution I hesitated for what seemed a 'minute' over locating the A. of C. (The subject thinks the hesitation was due to fatigue caused by fast walking just before the tests.) I. B., February 19: "If I could determine why the 8's and 9's bother I think I could overcome the habit of hesitating when I come to them." L. W. K., April 11: "Notwithstanding that the time of the second distribution was shorter than that of the first, I went timidly from box to box, hesitated to deliver the card, even though it was at the right box, this is apt to occur when I attempt to spurt. My habits seem able to bear only a certain rate of speed above which they break down, causing confusion and hesitation."

The anticipation of a troublesome card caused hesitation and dread to all subjects.

*Distraction*.—S. J. O., March 27: "Ran up against a wall when I reached the 8 of D.—hadn't the slightest idea of ever having seen it before. A few words spoken as I began the trial forced their way into consciousness, so that my attention was somewhat divided. I was so disturbed at not finding the 8 of D. box more quickly that I did not overcome the feeling until I had nearly finished the deck." L. W. K., February 20: "The heavy feel of time when I can not find the box is disconcerting, especially if it is a low numbered box. Search for face cards does not seem such a waste of time for I feel that I shall soon learn their positions." W. A. O., February 29: "Felt considerable of the nightmare desire *to do* coupled with inability. My hands were cold, and the cards did not pick up well. Was conscious several times of so thorough a disturbance of the habit that a card would look absolutely strange and novel as though I had never seen it before."

The most persistent and frequent sources of distraction consisted in a false delivery of a card, as falling on the floor, striking the sides of the box, tossing in the wrong box. Another source of distraction was the so-called 'clinker-cards'—cards for one reason or another difficult to locate. They caused distraction usually by appearing apparently unknown. If distracting experience did not fix them in memory they became sources of dread and hesitation.

*Strain and Tension*.—L. W. K., March 28: "Feel a heaviness before the case just as if I were lifting or jerking the case hither and thither instead of the card. My body sways needlessly back and forth while distributing." May 7: "Felt cramped, hurried and in the way of something or somebody, hesitated, made many false movements, made unnecessary mistakes, a feeling of humiliation, of distrust and of wanting to blame something for the poor records." W. A. O., March 23: "When I started the second time I was under tension and felt keenly my obligations to better my records. Things were going well when some one in the room strode by me and made some remark. I was just at sufficient hair-trigger tension to go to pieces—threw a black three with a red one, next time around saw it and fussed over whether to throw the next red

three in with it or not, felt a distinct check and a pronounced emotional reaction accompanied with heart pounding and throbbing at the temples." L. W. K., March 12: "Came to the task with too much tension and determination to make it go. The attitude lessened my powers of observation, for I overlooked several boxes and lost time thereby." Opposed to this condition is one in which the subject feels a lack of tonicity and tension. To supply the requisite amount one of the subjects in a droll mood would pinch himself considerably beyond the comfort point! He termed it "keying up."

Tension and strain were frequently accompanied by emotions with evident bodily reactions similar to the case above; a few additional cases are here submitted. L. W. K., June 15: "I was conscious of having made good time during the first trial. The second trial developed an emotion of unusual and surprising proportions. I set out with the resolve to beat my first record, and I was under high tension aroused by thinking throughout the week that I would make final records this morning so as to change my plan of work for next week. The distributing mechanism was unable to discharge the excess of nervous energy and at the same time function normally; the habit broke down, I became lost as to the number of the runs, allowed cards to fall out of the boxes, delivered two in wrong boxes, made false and superfluous movements and ending with voice, limbs and hands trembling." C. E. W., February 13: "This time I had a 'brain storm' as it were, I became confused (I do not know exactly why) and for about two minutes, I was unable to do anything it seemed. I let three cards fall. This increased the 'storm's' intensity, and soon another card fell—by this time I was so 'up in the air' that I scarcely knew what was going on except that everything I tried to do seemed to go wrong."

*Satisfaction and Elation.*—K. P., March 1: "I feel at'ease at those places where the boxes are known." W. A. O., February 17: "Felt pleased over the ease with which the cards fitted the new box. There was a sense of freedom every time a card went to place without careful planning, think the next time shall do it with some recklessness." L. W. K., March 9:

"The cards were distributed with the agreeable feeling that goes with assurance; with the exception of a few lapses my movements were deliberate."

*Confidence.*—L. W. K., March 5: "The feeling of confidence amounts at times to one of transitory elation, subsiding into a more permanent feeling of assuredness and certainty. My movements are deliberate, accurate, vigorous, often an over-charge of energy is used in delivering the cards: this may be a whimsical expression of victory or a rebuke to my previous wanderings—besides the snap or chug of the cards against the back of the box gives satisfaction." March 21: "Have reached a point in the process in which I can learn a definite thing or movement by making it a special object of regard. I have noticed this for the last few trials. Hitherto my learning has been uncontrollable. I learned what I could, not what I would. Now I learn what I will to learn."

*Fatigue.*—C. E. W., March 9: (The time of this distribution was fifteen seconds over that of the preceding.) "I think it due to the fact that I am mentally and physically wearied from lack of sleep and the worry and strain connected with reading examination papers, notebooks and reports. That strain being ended, I relaxed; and I seem unable to force myself to undertake any new task, even if a small one. I caught myself several times having 'thrown up the sponge' for an instant until my will ordered another attack." S. J. O., March 24: "Ran through the test after I had done my housework and was very tired. The first time I tried to make good time; and the second I felt so tired I didn't care about speed and just relaxed, feeling very calm throughout the run. The result was much more satisfactory."

Weariness and calmness were often associated in the same experience by all the subjects and at such times very often a new record of speed was attained, in one instance a subject's highest record was made at the close of a hard day's work. Since conditions of tension and surplus energy often gave poor results and those of weariness and calm comparatively good ones, it suggests the notion that a nerve current of over-high potential, as it were, breaks down a developing mechanism and

that weariness frequently means lowering the potential to the carrying power of the nervous pathways.

Concerning the affective tones, it appears that the milder ones occur at the beginning of the learning process when skill is at a minimum and again at the close when it has attained a maximum stage. (Facts contradicting the latter statement were obtained from subjects who had to close a series before a maximum stage had been reached.)

The more prolific sources of emotion are in the conditions just preceding the attainment of maximum skill or in those at the entrance to higher ranges of skill.

The intensity of an emotion is greater at an interruption of a habit than at the interruption of processes not yet reduced to a habit. The emotional response gives an evaluation of the performances and of the conditions out of all proportion to their intrinsic importance.

#### SUMMARY AND CONCLUSIONS

The conditions furnished by the playing cards and the distributing case for purposes of stimulating mental responses indicate many possibilities for minimal changes in plans of work, and involve comparatively simple factors capable of accurate description and control. The mental responses to the distributing situation represent a large field of conscious life and the concreteness and simplicity under which they occur and in which they appear bring the study of their origin, nature and functions within the range of the student.

Three classes of results grew out of the work: I. Those that verify known laws of mind, such as (a) serial learning, verified by learning the run of the cards; (b) inverse relation between imagery and habit, seen in learning the location of the compartments; (c) inverse relation between the intensity of attention and the amount of distraction, neatly proven by boxes on the perimeter getting first attention; (d) common laws of memory, *e. g.*, interest and recall (learning face cards first), emotional congruity and recall; (e) inverse relation between speed and error; (f) direct relation between intensity of feelings and impeded action. II. Those that are unique

and peculiar to the experiment itself, five stages of learning leading to maximum skill and related in a causal series: (a) learning the labels, (b) learning the compartments, (c) learning the run of the cards, (d) synthesising the 'runs' and the compartments and symbolizing them in several modes, (e) symbols and imagery of the synthesis give way to sensori- and ideo-motor and automatic processes. III. Suggested and partially solved problems: (a) Many of the errors of the second series were the correct distributions of the first. Such errors were most likely to occur when the speed was forced beyond the delivering power of the habit. Is this another case of ontogenesis copying phylogenesis in that an earlier structure functions for a later in the processes of disintegration? (b) The growth of the position-sense gives evidence that parts of it at least may go on outside the conscious plane. (c) Imagery, in learning under the above conditions, plays a minor role. (d) Interference of a motor segment proves a greater check to the learning process than a somewhat similar interference of a sensory segment.<sup>1</sup> (d) What is the origin and nature of a total memory lapse for a card apparently well known?

It would appear that the playing cards may be made well nigh as serviceable to the psychological laboratory as the guinea pig and frog have been to the laboratories of biology and physiology.

<sup>1</sup> The results of my 'Study in the Psychology of Spelling' (L. W. K., *Educational Psychology*, September, 1912) are apparently at variance with these. The wide difference in conditions and the unsolved experimental difficulties surrounding both these problems urges the necessity of further work before making final judgment.

REPORT OF EXPERIMENTS AT THE STATE  
REFORMATORY FOR WOMEN AT  
BEDFORD, NEW YORK

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In the summer of 1910, the writer spent six weeks in the New York State Reformatory for women at Bedford, for the purpose of carrying out some psychological tests upon certain of the inmates.

The object of these experiments was to find out (by the request of the superintendent) whether it would be possible to frame a practical set of tests which would, upon application to a given girl, determine whether she represented the grade of normality necessary to receive benefit from the educational work of this institution, or to be safely set free to earn her living after her term was over.

The aim of the experiments was wholly practical. Is it possible in this institution (for delinquent girls between sixteen and thirty serving a three-year term) to make use of an economical and convincing method for testing mental processes, in order that the deficient subjects may be at once given over to suitable custody, and may be saved the long term in an institution which is not appropriate for them, and the inevitable rearrest which follows the freedom which they cannot morally maintain?

The requirements of the tests were that they should be as simple and quickly applied as possible; that they should not alarm the timid, nor depend upon introspection. Girls in such an institution are for the most part unable to give accurate introspection, and furthermore are unwilling to attempt it, since it appears to invade a privacy which they employ every artifice to preserve.

Thirty-five girls were submitted as subjects, all of whom had been classified by the superintendent on a three-fold list. The lower grade girls were regarded by her as subnormal and

unfit for freedom. The list was not seen by the writer until the experiments were completed, and her own graded list submitted for comparison.

The tests included experiments in Reaction time, Memory, Attention, and direct and indirect Suggestibility. The different tests under each heading gave nine records in all. For each record a standard of normality was taken, and every girl who fell below the standard was marked as failing in the test. A girl who failed in six out of nine tests was regarded as subnormal.

The reactions were taken with a vernier chronoscope and a click stimulus. Ten trials were given for practise, and the average of forty subsequent trials was taken as the correct time. The average time for normal people is given as .14-.19 seconds.<sup>1</sup>

Any average reaction therefore of .20 or over, was regarded as subnormal speed. Eleven failed to reach this standard.

There were two memory tests, one auditory and the other visual. Two lists of nonsense syllables were used with three letters in each syllable. One list was read aloud to the observer till she could repeat it, and the other list was exposed at the same rate (two seconds exposure) one syllable at a time behind a small window in a screen.

A conservative average rate for women of memorizing such syllables is, twenty trials for an auditory series, and thirteen for a visual series.<sup>2</sup> The visual series is easier for the average woman who reads easily. Among the women at Bedford where reading is not a fluent accomplishment, and where the whole experiment was novel, twenty-five trials were taken as a fair standard in both sets of tests. If, after fifteen trials, there were so few syllables memorized that it was obvious that in twenty-five trials the list could not be complete, the observer was not fatigued by further effort. Any observer who had not learned the list before the twenty-fifth trial, was regarded as subnormal for either auditory or visual memory. When the observer was illiterate, the visual test was, of course, impossible.

<sup>1</sup> Titchener, 'Exper. Psychology,' instructor's manual, p. 216.

<sup>2</sup> Thompson, 'Mental Traits of Sex,' p. 94.

Seventeen failed in the auditory test, eleven in the visual memory test, and seven were unable or unwilling to take the latter test.

There were three attention tests. In the attention span test, the observer was shown a set of seven cards  $6 \times 2\frac{1}{2}$  inches, upon which were pasted in all eighty-six objects, such as pictures, letters and scraps of colored paper. The cards were exposed three seconds, and the observer was asked to tell what she had seen. A record of only 21 per cent. and under of the whole number exposed (*i. e.*, an average of less than three objects on a card) was regarded as subnormal.<sup>1</sup> Nineteen failed to pass this test.

The second attention test was one of distraction.<sup>2</sup> The observer was asked to run a small pointer as quickly as possible over an involved maze of lines, and her speed was taken from an average of seven trials. Then a similar maze was provided which had pictures and other distracting objects pasted between the lines, and the average of seven more trials taken, to find how much the pictures had distracted the attention. The filled and unfilled mazes were alternated, so that one would not have all the advantage of the practice gained upon the other. 95 per cent. of control and under as measured by time difference was regarded as subnormal; also a failure to traverse the whole maze<sup>2</sup> after a fair amount of practice in less than 150 seconds. Fourteen failed on one account or the other to pass this test.

The third attention test was simply counting the number of letter O's in a paragraph of fairly fine print. The letter actually occurred fifty-five times, and 70 per cent. or under, of correctness from an average of five trials, was called subnormal. Sixteen failed in this test.

The last three tests were in suggestion. In the first, the same cards were used as in the attention span experiment. After the observer reported all the objects which she remembered to have seen on the cards, she was asked if she did not remember such and such things, none of which were

<sup>1</sup> Whipple, 'Manual of Mental and Physical Tests,' p. 247.

<sup>2</sup> Burnett, 'A New Test for Attention against Distraction,' PSYCH. BULLETIN, February 15, 1910.

actually present. 20 per cent. and over of successful suggestions were classified as abnormal, and fifteen failed to pass the test. In this case it was impossible to determine whether the subject actually believed she saw the object to which she assented, or not. In general, however, the girls were so much impressed by the experiments, and approached them with so much seriousness, that it can safely be assumed they tried to do their best.

In the second suggestion experiment, ten cards were shown, on each of which were pasted a pair of equal white circles, with unequal numbers written across their faces.<sup>1</sup> The girls were asked after seeing each card, which of the two circles was the larger. If 70 per cent. of the judgments and over were that the circles with the larger number were larger in diameter, the observer was marked as abnormally suggestible in that experiment. Where the girl could not read, the numbers were read to her. Sixteen girls reported seven circles as larger, in which was the larger number.

In the last test, the observer was shown, one by one, a set of twelve lines, the first five of which increased progressively in length by twelve millimeters, and the latter seven increased in equal pairs.<sup>2</sup> The observer was asked to reproduce each line as it was shown to her, and if she continued increasing the later equal lines, because the earlier unequal lines had been so increased, she was considered suggestible in that particular. Any coefficient of 75 per cent. and over was called abnormal. Nineteen failed in this experiment.

As a result of these nine classified tests, eleven of the thirty-five experimented upon were found subnormal; that is, they failed in six of the nine tests. Upon comparison, this grading tallied with the estimate formed of their capacity by the superintendent, with the exception of two girls, who failed only in five instead of in six tests, and hence did not fall into the lowest grade of the experimenter. However, since neither of them was able to read well enough to even try the visual test, both had been tested in only eight instead of nine experiments.

<sup>1</sup> Münsterburg, 'Psychology and the Teacher,' p. 180.

<sup>2</sup> Binet, 'La Suggestibilité,' p. 106.

This inability of some subjects to read easily complicates any experiment in which there is a demand on the observer to report a printed letter. It is very difficult to determine how much a girl is prevented from reading by inability, and how much by obstinacy and a disinclination to exert herself. Sometimes there is a failure to report an object seen because of the limited vocabulary of the observer, and a preference for saying that she did not *see* the object, to admitting that she cannot think of the word. With foreign-born girls, as many of these were, the problem becomes even more complicated. But in spite of all these difficulties, such a set of tests as this accomplished the purpose of classifying eleven subnormal girls by an objective standard in a relatively short space of time. There were more variations between the list of the superintendent and that of the experimenter in the grading of the first and second class girls, that is, those who had three to five failures, and those who had two or less. But even those agreed in the main. Three girls passed every test successfully.

Later in the year eight of these tests were tried upon thirty-five students in Mt. Holyoke College, and seven tests (by the kind assistance of Dr. Toll) were tried upon as many students in Amherst College. The following tables give the comparative list of the numbers failing in the tests in the three institutions.

1. Reactions	5. Attention control in counting o's
Mt. Holyoke, 5	Mt. Holyoke, 0
Bedford, 11	Amherst, 0
	Bedford, 16
2. Memory. Auditory	6. Direct Suggestion
Mt. Holyoke, 0	Mt. Holyoke, 3
Amherst, 0	Amherst, 2
Bedford, 17	Bedford, 15
3. Visual	7. Circle Suggestion
Mt. Holyoke, 1	Mt. Holyoke, 9
Amherst, 0	Amherst, 4
Bedford, 11	Bedford, 36
(7 could not take it)	
4. Attention Span	8. Line Suggestion
Mt. Holyoke, 0	Mt. Holyoke, 4
Amherst, 0	Amherst, 2
Bedford, 19	Bedford, 19

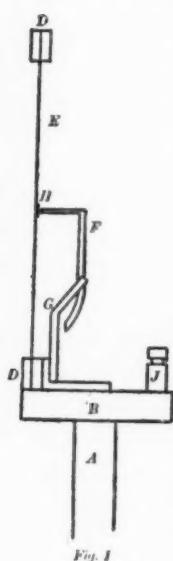
A resident psychologist has since been installed at Bedford.

## APPARATUS FOR ASSOCIATION TIMING<sup>1</sup>

BY KNIGHT DUNLAP

The combination of apparatus for the timing of association reactions which has proved in the Johns Hopkins laboratory very satisfactory consists of (a) an Ewald chronoscope, (b) a 64 d. v. electric fork with two contacts mercury,<sup>2</sup> (c) two voice

keys somewhat similar to the "Schallschlüssel nach Roemer," but simpler (Fig. 1), (d) a double relay of my own design (Figs. 2 and 3), and (e) a master switch, which I designed and have used some time for general purposes, but which has not been described in detail (Fig. 4). In addition to these specified pieces, suitable current sources and rheostats are used.



The voice key is shown in partial lateral projection in Fig. 1. It consists essentially of a diaphragm *E* (at present of paper), held in a split ring *D*, which is mounted on the hard rubber base *B*, attached to a rod *A*, by which the instrument is held in a tripod. The diaphragm *E*, carries at its center a piece of platinum foil *H*, against which rests the gold tip of the light lever *F*, pivoted in the support *G*. *H* is connected by a fine wire (not shown) to the binding post *J*, and *F* is connected to a similar binding post. *F* is connected to a similar binding post.

The diaphragm, in vibrating, repels the lever *F*, breaking the contact, which is reestablished when the vibration ceases.<sup>3</sup>

<sup>1</sup> From the psychological laboratory of the Johns Hopkins University. The apparatus, with the exception of the fork, was exhibited at the Cleveland meeting of the American Psychological Association.

<sup>2</sup> The 64 d. v. fork is used because we happen to have it. A 50 d. v. fork would be most suitable for normal work; and the 10 d. v. reed exhibited with this apparatus at Cleveland is satisfactory for clinical work.

<sup>3</sup> There are really a series of makes and breaks during the continuation of the vibration, but as the key is used, the first break, even if of but a few sigma durations, does the work. I shall probably modify the contacts so that the circuit remains

The key is extremely sensitive and responds to conversational tones with the mouth from six to ten inches from the diaphragm.

The double relay is shown in two projections in Figs. 2 and 3. An armature *E*, pivoted in the piece *A*, is acted upon by the two electro-magnets *C* and *D*. The magnets are as nearly alike in cores and windings as it was possible to make them. The two magnets being inserted in parallel in the same circuit, the armature will remain at either side of its swing, against contact *J* or contact *H*, since the poles to which it is the nearer will exert the stronger pull. Suppose now the stimulus voice key is connected in the branch of the circuit through magnet *D*, and the reaction key in the branch through *C*, and the armature is against contact *J*. Speaking the stimulus word interrupts momentarily the current through *D*, allowing the armature to move over to *H*, and speaking the reaction word, breaking the circuit through *C*, allows the armature to move back to *J*. Since the latent period in each magnet is the same, the error in the time is negligible.

The Ewald chronoscope, and one contact of the fork, being connected in series with a current source, the armature through binding post *K*, (Fig. 2), and contact *H* through binding post *N*, the chronoscope runs during the period of contact between the armature and *H*, and hence registers in 64ths of a second the time between the beginning of the stimulus word and the beginning of the reaction word.

Because the voice keys are very sensitive, the current cannot be turned on the magnet circuits until just before the stimulus word is given. It is also necessary that the circuit be completed first through the stimulus key, and then through the reaction key, in order that the armature may automatically take the correct position. Hence a master key, which shall complete the two branches of the circuit in the order named, with one movement of the experimenter's finger, is required.

broken during the whole of the time the diaphragm is vibrating; although this does not now seem necessary.

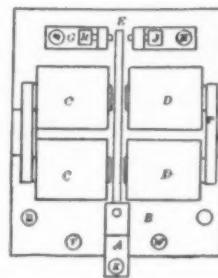


Fig. 2

The key illustrated in lateral projection in Fig. 4 is designed to do this, and fills this and other requirements.

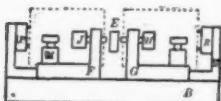


Fig. 3

As the key is represented (Fig. 4) it is adjusted to perform a more complicated function than is here required. On depressing the finger button *A*, the lever *M* (pivoted at *P*) first descends, breaking contact at *F* and making contact at *V*;

then the lever *L* (pivoted at *R* in a frame carried on the other lever, but insulated from it) rises, and makes contact at *S* at the same instant that the contact at *V* is broken. This adjustment of the key is for spark-chronograph work, and for other work with the induction coil spark. In using the master key in the present arrangement, the spring, *G*, is removed, and the lever, *M*, is then normally in contact with *V*, instead of with *F*. Pressing the button therefore first makes contact at *S*, and then at *F*.<sup>1</sup> The undivided voice key circuit is led

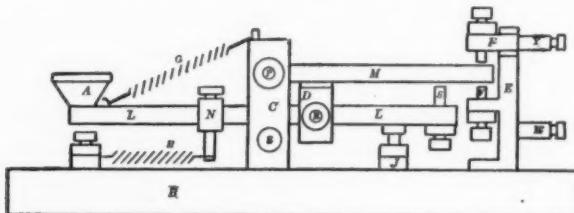


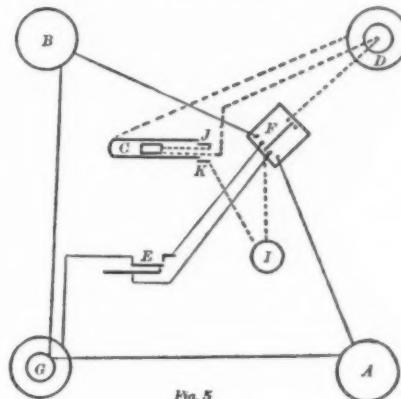
Fig. 4

to lever *M*, through a binding post not shown; the stimulus key branch is drawn off from *S*, and the reaction key branch from *T*, through binding posts *z* and *y* respectively.

The method of connecting the various pieces of apparatus is shown in Fig. 5, in which the circuit through voice keys and relay magnets is represented by the solid lines, and the circuits through the Ewald, electric fork, and the relay contact, by the broken lines. In Fig. 5, *G* and *D* are current sources (I actually use the 120 volt d. c. lighting circuit, with lamp

<sup>1</sup> The method of attaching the spring *H* to the lever *L*, is, I think, new. The spring-tension is increased by sliding the clamp *N* along the lever towards the pivot. As a long spring can be used, most satisfactory adjustments can be obtained. From all points of view, this is the best method of attaching springs to key levers.

resistance and sliding rheostats in parallel to reduce voltages); *A* is the stimulus key, and *B* the reaction key; *E* is the master key; *F* the relay (the magnet terminals and armature only being represented); and *C* is the fork, with contact *J* controlling



the circuit through the fork magnet, and *K* the circuit through the Ewald.

The apparatus, once adjusted, requires little attention and has been used successfully by students who have little mechanical skill. The routine of use is simple: the mercury contacts of the fork must be supplied with alcohol (applied with a dropper every few minutes); the Ewald must be turned back to zero before each reaction; the master key must be pressed just before speaking the stimulus word, and held down until after the reaction: that is all.

## A COLOR-TRIANGLE FOR LECTURE PURPOSES

BY M. LUCKIESH

*Physical Laboratory, National Electric Lamp Association*

It is a fact easily demonstrated that any color can be accurately matched by combining certain amounts of the primary colors—red, green and blue. Hence in order to graphically compare colors it is customary to plot their primary components in trilinear coordinates. This results in a triangle enclosing an infinite number of points each of which at least represents a theoretical color. It is customary to show this by a lantern slide or chart in black and white.

The writer recently hit upon a simple method of showing this color triangle in actual colors and it has proven so valuable for lecture purposes that he feels sure psychologists will be interested. A box 6 inches in depth and whose section forms an equilateral triangle about 18 inches on a side is made of wood with a wooden back containing vent holes. A ground milk-opal glass in the form of an equilateral triangle somewhat smaller than the section of the box forms the front side. In the three corners of the box are placed a red, green, and blue tungsten lamp respectively. With proper adjustment of the lamps the diffusing glass takes on the colors of a color triangle and a close approximation can be approached depending on the care exercised in adjusting the position of the three lamps. The most important part of the apparatus is the diffusing glass forming the front side. This is best produced by grinding a flashed-opal glass. The lamps which should be of a standard voltage can be placed on any lighting circuit and conveniently controlled by a switch. When the lights are suddenly switched off the after-images are very striking, especially with daylight illumination. The apparatus has proved very valuable for color demonstration.

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